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THE DEBT OF PHYSICS TO METAPHYSICS[1]

IF I venture to address this society upon a subject where I am very liable, perhaps even likely, to be misunderstood, please bear in mind that I do so only in the belief that it is a matter of no small importance for workers in any one science to realize fully the limitations as well as the powers of their own science. It is hardly necessary to add, that while I shall consider a phase of physics which has little to do with experiment, I am not for an instant unmindful of the fact that ours *is* an experimental science, and that all the really great achievements in physics have been wrought through, or have led up to, or have been completed by, experiment and observation. This remark is doubtless true even of the supreme work of Newton, Fresnel and Maxwell. Nor am I forgetful that in days gone by the normal development of sound physics has been much retarded by metaphysics.

Second to none in my admiration of the man who has contributed even a single experimental fact to either the foundation or superstructure of the edifice which we call modern physics, I invite your attention for a few moments to the debit side of the account as it stands between the physicist and metaphysician. This I do with no little trepidation, remembering how easily one may, even with the utmost good will, go astray in a strange field of thought. Metaphysics is a term employed with such a variety of meanings that I must, at the very outset, explain the one sense in which

[1] Presidential address before the American Physical Society in New York, March 5, 1910.

I am using it. I am *not* employing it to indicate "the sum of all knowledge" (Paulsen), or as a synonym for the "science of the absolute" (Hegel), but rather as a branch of philosophy which is, in a certain sense, supplementary to all the individual sciences of phenomena. The metaphysician here in mind is a gleaner after physics and psychology, using these two words in their wide meaning so as to cover practically the whole of modern science. He it is who orients the sciences among themselves, criticizes their foundations, their methods and even their conclusions, in so far as these conclusions depend upon pure logic. He it is who rounds out and corrects the individual sciences. It will thus be seen that metaphysics, with these limitations, does not differ widely from the modern usage of the word philosophy; for the metaphysics I have in mind has been aptly characterized as "the supreme science of order."[2] There are those, and I myself am one of this class, who prefer to use the word "epistemology" to describe a metaphysics of this type. It is certainly not the type of metaphysics which allowed Kant to define matter in terms of force.[3] And, in any event, I trust we shall all agree that we are not getting into what Maxwell called "the den of the metaphysician, strewed with the remains of former explorers, and abhorred by every man of science."

But we must be careful to remember that the metaphysician which Maxwell here has in mind is not an epistemologist, but a man of the Hegelian type. Helmholtz[4] boasted that he never lost an opportunity to impress upon his students the principle that "a metaphysical conclusion is either a false conclusion or a concealed experimental conclusion." How far removed the more genuine metaphysics of to-day is from that which held sway during the first half of the nineteenth century and which exasperated men of the type of Maxwell and Helmholtz, may be indicated by the following paragraph from Professor A. E. Taylor,[5] of Aberdeen, himself a distinguished metaphysician. He says:

> Just because of the absence from metaphysics itself of all empirical premises, it can be no business of the metaphysician to determine what the course of events will be or to prescribe to the sciences what methods and hypotheses they shall employ in the work of such determination. Within these sciences any and every hypothesis is sufficiently justified, whatever its nature, so long as it enables us more efficiently than any other to perform the actual task of calculation and prediction. And it was owing to neglect of this caution that the *Naturphilosophie* of the early nineteenth century speedily fell into a disrepute fully merited by its ignorant presumption. As regards the physical sciences, the metaphysician has indeed by this time probably learned his lesson.

It is hardly necessary to add that the type of metaphysics here exposed is not one to which physics owes anything whatever, and is not the one I have in mind during these remarks.

I. THE MECHANICAL POSTULATE

The father of the present Duke of Argyll rendered marked service to science in pointing out how wide-spread is the use of physical and natural law. But nowhere in his notable volume, the "Reign of Law," does he indicate what may be called the most fundamental fact connected with the discovery and employment of such law, namely, that the very existence of laws governing natural phenomena is a postu-

[2] Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 236.

[3] Hoeffding, "History of Modern Philosophy," Vol. 2, p. 69.

[4] Vorträge, "Das Denken in der Medicin," p. 34.

[5] Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 240.

late laid down, consciously or unconsciously, by the investigator. No one, except the later metaphysicians, has convinced us that however tangled the knot of physical facts which we are called upon to explain, the first thing we *assume* is that these phenomena are subject to law. We assume that we are studying a machine which behaves in a definite manner.

This assumption—which we may call the mechanical postulate—is not something to be discovered or verified by experiment, not something whose adoption stamps a man as a materialist, not something which sane men consider, in order to accept or refuse, but something which *all* men adopt as a laboratory convenience, one might better say, a laboratory essential.

Nor is the mechanical postulate one which is confined to physics; but is employed in all the sciences where men are attempting to bring order out of chaos. It is not, therefore, something to be charged up against one in the sense employed when modern physics is said to rob the world of all spontaneity and sentiment, or when science is said to be devoid of poetry. While we treat nature as a machine and while we adopt the mechanical hypothesis as a necessity of productive scholarship let us be very careful however not to allow ourselves to dogmatize to the extent of saying that a machine is all we have.

Is not the physicist under obligations to the philosopher for making this matter perfectly clear?

Apparent deviations from mechanical law lead to some of the most important biological problems. Animate and inanimate matter may appear, at first glance, to belong in two different categories; and so they undoubtedly do as regards many of the superficial phenomena. But conversation with some of the most productive scholars of our country in zoological and botanical lines has convinced me that they are practically all working on the assumption that biological phenomena are physical phenomena. These investigators assure us, moreover, that the introduction of an *enteliche* here and there, wherever convenient, would be sufficient to discourage all serious research on life problems. The same point of view is expressed by Münsterberg when, in his classification of knowledge, he places physics and biology together at the very bottom of the group called "physical sciences."

The hatching of an egg is apparently a different process from that of melting ice, although both are accomplished by the application of heat. But to assume anything else than that they are both mechanical processes is merely to erect a barrier which shall delay the discovery of truth. The study of cytology and artificial parthenogenesis have already gone so far that the discovery of a much more definite connection between life and mechanics would shock the world perhaps even less than did Wöhler's synthesis of urea in 1828.

UNIFORMITY POSTULATE

There is of late a very distinct change of feeling in regard to the principle of the Uniformity of Nature—a principle which was widely circulated, a generation ago, as an experimental fact but which is now properly regarded as another formulation of the mechanical postulate. But, thanks to the metaphysician, this principle is now, so far as I know, regarded by us all, neither as an axiom nor as an empirical fact, but as a fundamental hypothesis which we may call the "uniformity postulate."

This assumption is practically equivalent to considering matter, energy and electrifi-

cation to have no personal or individual traits which we need take into account. Without this postulate we should be unable to generalize our physical laws so as to include many new phenomena—phenomena unknown at the time of the formulation of the law. The tenacity with which the experimentalist holds to his assumption of a simple law is well illustrated by two papers read before the last meeting of this society: papers which illustrate how complex nature is becoming as research goes on. I refer to the work of Professor H. W. Morse and Professor E. B. Rosa on electrolysis. Each investigation dealt with slight deviations from one of Faraday's fundamental laws: and each investigation apparently assumed its truth: in any event assumed an equally simple law. Thousands of engineering results obtained each day in the week convince us that there are no accidents in history and allow us to believe that no postulate was ever better justified by its success.

The behavior of nature in this respect always reminds me of a remark, really a new formulation, once made by Professor Michelson in describing the labor of several years in locating and eliminating the errors in a certain steel rod upon which he was cutting an accurate screw. "I felt," he said, "as if matched in a game against an opponent: *but my antagonist always played fair.*"

II. ENERGY POSTULATE

Passing now to the consideration of energy, it is not yet three score years and ten, since Poggendorff and Magnus refused space, in the *Ann. d. Physik.*, to Helmholtz's little tract, "Die Erhaltung der Kraft," on the ground that it was too metaphysical. But thanks partly to the clear vision of Helmholtz, partly to the clever analysis of H. Poincaré, and largely to the experimental success of the principle, the time has now come, I believe, when we can say that the conservation of energy is so useful, *as a postulate*, that present-day science can not successfully accomplish its work without it. Experiment has been able to demonstrate it as a law only for particular cases and only approximately: but experiments have been so numerous and compelling, as to have created a new attitude of mind in the present generation, leading us to believe that everywhere in the physical universe there is *some* constant quantity, corresponding to a certain constant of integration, called "energy."

The most recent illustration of the manner in which the physicist assumes this constancy is, of course, the case of the steady heat production in radium. No sooner had Curie and Laborde made this remarkable discovery, in 1903, than men began, *not* to doubt the validity of the law of the conservation of energy, but to look about for the energy which was thus being transformed into heat. Accordingly Rutherford and Barnes succeeded, in the following year, in showing that 23 per cent. of this intra-atomic energy was due to radium itself, 32 per cent. to radium C and 45 per cent. to the emanation and radium A together. In saying that the time has come when the Law of the Conservation of Energy may properly be regarded as one of the presuppositions of physics, it is to be carefully noticed that this statement does not include the Law of the Dissipation of Energy.

III. CAUSAL POSTULATE

The infinite regress involved in the search after causes and the vanity of attempting to follow a series of causes to its end are, at least, as old as the Greeks.

The postulate which the philosopher here shows us to be one of our presuppositions is as follows: events in physical science depend upon a few antecedents, knowing which we may successfully predict the immediate consequence, and may safely disregard all other circumstances. The brevity of the sequence which really determines phenomena in physics is a matter of continual surprise—while the length and complexity of the sequence in the case of ordinary human actions is a matter of equal astonishment.

But it is very easy to forget what a powerful influence this postulate has at times exerted in almost all departments of science. Few physicists, and still fewer engineers, of the present seem to realize that some of the most fundamental conceptions of our science have been introduced directly through the adoption of this postulate.

Take, for instance, what is perhaps the central idea of modern dynamics—the idea of force—an idea which is older than either that of mass or of energy. When viewed in the light of the causal postulate, *i. e.*, in the light of history, the definition of force becomes a matter of the utmost simplicity and perfect clarity. From many other points of view it is one of the most complex and puzzling of physical quantities. Sir Oliver Lodge says:

> We are chiefly familiar, from our youth up, with two apparently simple things, *motion* and *force*. We have a direct sense for both of these things. We do not understand them in any deep way, probably do not understand them at all, but we are accustomed to them. Motion and force are our primary objects of experience and consciousness; and, in terms of them, all other less familiar occurrences may be stated and grasped.

To identify "force" in this manner with the "muscular sensation" of tension or pressure, which we feel when giving an accelerated motion to a body or when equilibrating by muscular effort the pull of the earth upon a body, seems to me dangerously near darkening counsel with words, and quite contrary to the spirit of the modern mathematician and physicist who are mending their fences at every possible point to keep out ideas which are not clear, sharp and definite.

The standard definition of the engineer, and, I fear, of not a few students of physics, is set forth by Professor William Kent in his article on the teaching of dynamics which appeared in SCIENCE[6] a few weeks ago, namely, "Force is defined as a pull or push, something that causes or tends to cause either motion or a change in the velocity or direction of motion."

Now considering both of these points of view, which I believe are widespread, every one is willing to admit at once the existence of certain elastic, and gravitational, and muscular, and electric, and cohesive, stresses which none of us understand: but the historical, or, if you please, the metaphysical, point of view would appear to be something like the following.

So far from our possessing any direct muscular sense of force, in the physical meaning of the word as distinguished from muscular tension, with which we are all familiar, the idea is one which was introduced by an Italian professor of mathematics, but a comparatively short time ago. How short may be illustrated by the following circumstances:

My grandmother, who lived in my own home for a number of years, was born on the banks of the Brandywine in 1789. She was therefore a contemporary as well as a neighbor of Benjamin Franklin. When Franklin was a printer's lad in London he had a promise from a friend that he should be taken to visit Sir Isaac Newton. Sir Isaac Newton was born within the same week in which Galileo died. Two human

[6] SCIENCE, Vol. 30, p. 919, 1909.

lives suffice therefore to bridge the gap between Galileo and our contemporaries. Back to Galileo is not therefore a far cry.

Recognizing the limitations of his science, and seeing that the search after causes was futile, Galileo adopted the causal postulate and prepared to confess his ignorance of gravitation, cohesion, muscular tension, and to say that, when we see a body changing its momentum, there is a "force" at work upon it. Following is the sentence, from his "Dialogues"[7] in which he introduces force as a synonym for any of these unknown influences which produce acceleration:

> It does not appear to me worth while to investigate the *causes* of natural motion concerning which there are as many different opinions as there are different philosophers. Some refer them to an attraction towards the center; others assign them to repulsion between the small particles of a body, while still others would introduce a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now all these fantasies, and others too, must be examined; but it is not really worth while. For all that is needful is to see just how one investigates the properties of accelerated motion and how these are defined, *without consideration of their cause*, in such a way that the momentum (of the body) increases uniformly from the initial condition of rest in simple proportionality to the time.

The paragraph which I have just quoted is, so far as I am able to learn, the earliest expression and definition of that central physical quantity which we now call "force." Observe first of all the modesty of the man; twice within this definition he inserts a distinct disavowal of any consideration of the cause of motion. So far is he in advance of our modern text-books, that he declines to define force as a "cause of motion" or a "tendency to produce motion," but says it is not even worth while to consider the question from that point of view.

How clear these same ideas were to Newton will be evident from the following two sentences from the first book of the "Principia." He says:

> For I here design only to give a mathematical notion of those forces without considering their physical causes or seats.

And again:

> Wherefore the reader is not to imagine that by those words I anywhere take upon me to define the kind or the manner of any action, the cause or the physical reason thereof.

Having thus abandoned all consideration of cause and having assigned ourselves the simpler task of describing the motions of bodies, we come back to the definition of Galileo and Newton, namely, the rate of change of momentum—as the one perfectly *correct, competent* and *complete* description of force.

It remains only to show that Galileo had a clear and modern conception of momentum. This is sufficiently evident from the following paragraph in the "Dialogues."[8] He says:

> It is clear that an impulse is not a *simple* matter, seeing that it depends upon *two* important factors, namely, the weights (il peso) of the colliding bodies and their velocities.

And again on the same page he says

> It is customary to say that the "momentum" of a light body is equal to the "momentum" of a heavy body when the velocity of the former bears to the velocity of the latter the inverse ratio of their weights.

If then I have correctly stated the facts of the case, force would appear to be a pure concept of the intellect: but a precious concept; one which is well understood, clear, definite, quantitative, and one whose extraordinary usefulness has made

[7] Ostwald's "Klassiker der Exakten Wissenschaften," No. 24, p. 15.

[8] Ostwald's "Klasiker der Exakten Wissenschaften," No. 25, p. 44.

it survive through the entire history of physics.

The paradoxy of this dominant idea of modern physics being a mere picture created by the human mind, disappears when we consider how the same method is employed in subjects other than physics.

In history, for example, we have important culminating events which we ascribe to "certain influences," while as a matter of fact the most that we actually know and observe in history is a series of *individual* acts, prompted, we suppose, by certain purposes.

The Franco-Prussian war came when the German Kaiser decided to send the telegram from Ems, when Prince Bismarck decided to publish certain parts of this telegram, when Von Moltke decided that the army was ready, when Napoleon III. decided to emulate the military career of his uncle, when the Congress of Vienna decided, in 1815, to give Prussia additional Rhenish territory, when in 868 the father of Lothar gave to his son the middle kingdom, the modern Lorraine, between France and Germany.

In practise we find it more convenient to say that "certain influences" had been at work for a full thousand years which culminated in the victory of Prussia over France. In physics, we give to the corresponding "influences" the name forces. That's the whole story! We *measure* these influences by the mass-acceleration of the body under consideration.

The extension which this idea of force has received in later times is known to us all. Huygens was the first to show that Galileo's fundamental variable, linear momentum, might change in two ways, namely, in direction and amount; and he gives us for the first time a method of computing the force when the momentum varies in direction only—a force which we now call "centrifugal." Later, in the case of rigid bodies the conception of "angular momentum" was introduced; its time variation we now call either "torque" or "precessional couple" according as the angular momentum varies in amount only or direction only.

This definition is identical in form and meaning with that of Galileo.

The essential step made by Lagrange, in his treatment of the simplest possible case, namely, a single particle, is to derive both the time variation of momentum and the rate of directional change of momentum, each by differentiation of a single function.

Momentum for him is the velocity-variation of kinetic energy, a quantity whose time-variation is the tangential force; and centrifugal force is the space-variation of kinetic energy: but each of these is still a time-variation of momentum, agreeing perfectly with Galileo's original definition.

The space-variation of *potential* energy is the measure of stress—or more properly a stress integral—which we do not understand—but which nevertheless can be evaluated in terms of force.

I shall detain you for only one more illustration.

Faraday had discovered a quantity—the "electrotonic state," he called it—electro-kinetic momentum, we call it—whose variations through any closed circuit, were always accompanied by an electric current in that circuit. Not knowing the cause of this current, physicists agreed to say that an "electromotive force" was at work whenever the electro-kinetic momentum changed, and to define this electromotive force as the time rate of change of electro-kinetic momentum (Neumann). Here again we have a generalized force introduced as a synonym for an unknown cause; exactly as was done by Galileo in the first instance.

Let us distinguish carefully between the observed facts of nature and those tempting pictures of the human mind which we only too easily create and are only too apt to worship.

Among the realities of mechanics are to be mentioned bodies in motion, liquids flowing, springs changing length; among the abstractions of the subject—helpful and needful abstractions—but abstractions nevertheless—are to be numbered the forces, velocities and accelerations of these bodies. Only by understanding these matters and by drawing a sharp line here shall we avoid Maxwell's "den of the metaphysician."

It is not infrequently that one finds a clever metaphysician in the orthodox man of empirical science; and I am free to confess myself unable to say whether the majority of the criticisms of the foundations of our science are due to the physicist or the philosopher; but in either case the critic speaks *as a metaphysician*. As an illustration consider the penetrating criticisms of the foundations of rational dynamics recently given by Mr. Norman Campbell,[9] who shows that the science of mechanics is so loaded with assumptions that the experimental verification of its laws is utterly hopeless.

IV. PRELIMINARY DISCUSSIONS

Fourthly, metaphysics has, I believe, rendered distinct service in giving us certain helpful preliminary discussions. Indeed, it is the history of many of the special sciences, such as psychology and sociology, that they were at one time departments of philosophy—but now, having shown themselves amenable to experiment or observation and subject to the "reign of law," are established as kingdoms of their own. The very notion of mechanical law is at least as old as Thales—600 B.C.—whose idea it was, in common with Anaximander, Anaximenes and Heraclitus, that the variety of things is due to "a single material cause, corporeal, endowed with qualities and capable of self-transformation."[10] Ridiculous and absurd as this sounds to us, it nevertheless contains the fundamental conception of mechanical law, and made it easier for later men to adopt more useful hypotheses.

The history of the atomic theory illustrates well the value of this contribution. The atom of Democritus—a purely metaphysical structure—differs in no essential respect from the modern atom up to the year 1738 when Daniel Bernoulli initiated the kinetic theory of gases.

The contention of Anaxagoras that all bodies are really continuous has also been of the utmost help: Poisson adopted it *in toto* in his mechanics; it was employed in electrical science up to the date of Helmholtz's Faraday lecture, 1881, and it is to-day practically adopted in all discussions of hydrodynamics.

Maxwell[11] goes so far as to say:

> In the earliest times the most ancient philosophers whose speculations are known to us seem to have discussed the ideas of number and of continuous magnitude, of space and time, of matter and motion with a native power of thought which has probably never been surpassed.

It was a really profound insight into the nature of pure mathematics that led certain participants in the relativity discussion, at the last meeting of this society, to place in the same class the metaphysician and the mathematician; the new grouping of studies at Harvard College does the same; each of these subjects is concerned neither with phenomena of any kind, nor with individual purposes, but with those

[9] *Phil. Mag.*, January, 1910.

[10] "Encyclopedia Britannica," 23, 219.

[11] "Encyclopedia Britannica," art. Atom.

over-individual purposes, with those universal agreements, with that world-wide consensus of opinion, in which all sane men unite; in brief, mathematics and metaphysics each belong in the group which Münsterberg calls the "normative sciences." There is therefore a certain sense, which in passing I merely mention, but do not urge, in which all consideration of number and quantity and limits which the mathematical philosophers have handed down, increases the debt of physics to metaphysics.

Sound method in drawing inferences is a branch of science to which the physicist owns no copyright, but one in which he may claim to be fairly well versed. For this method he is indebted in no small degree to the development of logic in the hands of the metaphysician. In brief, modern physics, at its very inception in the seventeenth century, found that the schoolmen had already furnished it with a set of beautiful tools in the shape of fundamental logical ideas, including "precise definition," "classification," and "fallacies." Even Bacon when "preaching the funeral sermon of scholasticism," used the accurate methods of the schoolmen.

Space and time, as continuous quantities and as limiting conditions for all phenomena, is another conception of no small value which we have inherited from the Greeks. The critical examination of our conception of time, which was given by Einstein[12] some five years ago, and perhaps even earlier by Lorentz, had, among other interesting and more valuable features, the following: He showed clearly —and, so far as I am aware, for the first time—just what kind of "time" we have been and are still using in ordinary Newtonian mechanics, namely, time such as would result from having all our clocks controlled by a single central time-keeper *which would transmit its controlling signals with absolute instantaneity.*

The clear definitions of synchronous clocks and simultaneity—in brief the idea of local time—may be considered as belonging either to physics or to mathematics —but surely the exposition in which Einstein has taught us just what kind of time we have been unconsciously using for more than two centuries is a metaphysical contribution of high order.

The dangers of mere nominalism, or, if you prefer, extrapolation, by which I mean the danger of ascribing to any physical system a set of properties which we have merely learned to associate with its name, has been clearly pointed out in the history of philosophy. Due regard for this warning would, I believe, have saved many pages that have been written concerning the ether—especially those devoted to a determination of its inertia, its weight, and its place in the periodic table of Mendelejeff.

V. LIMITATIONS OF SCIENCE

Fifthly and lastly the metaphysician has rendered the inestimable service of pointing out to the experimental investigator the paradox that his greatest strength lies in his confessed limitations. Each of the particular sciences views phenomena from its own particular angle; but there is, I fear, sometimes—often, indeed—a tendency for the student of physics to think that in measuring, say, the inertia of a body, he is in some sense getting at the "quantity of matter" in it; or to put it in another way, there is often a tendency to think that in determining the mass, on a beam balance, he is perhaps doing something more fundamental than merely determining inferentially the ratio of the

[12] *Ann. der Physik* (4), 17, 891–921 (1905).

inertia of this body to the inertia of some body selected as a standard; for which purpose he has abstracted the inertia from all other properties of body and is really no nearer the nature of the ultimate "substance" of the body than if he had measured its temperature or its color.

A most important limitation which might have been entirely forgotten were it not for the metaphysician, is the fact that *phenomena* do not constitute the *entire* subject matter of science. Indeed it is only the mental and physical sciences which deal with phenomena. Human purposes and acts of the human will are quite as much subjects of scientific study, whether we consider the individual, the group or the entire race of sane men, as are any of the phenomena of physics. It includes such branches as history, politics, language and literature. Not only so, but if we define the real as that with which we must reckon in the accomplishment of our purposes, this second group of sciences deals with subject matter which is quite as real as anything we consider in physics.

It will perhaps not be out of place here to repeat the warning given by President Maclaurin[13] to the American Chemical Society on the occasion of the recent Boston Meeting of the American Association for the Advancement of Science. He says:

> We should pay more serious attention than we usually do to the logic of science and have as clear ideas as possible as to what we are really aiming at, as to what we can really expect to do and not to do. A little artificial stimulus toward philosophy might accelerate the process. It seems to me extremely unfortunate that men of science are still so much scared by the bogey of metaphysics. . . . We should realize, perhaps, that a science such as chemistry is above all else a work of art, and that concepts like atoms, energy and the like are not much more than pigments with which we paint our pictures.

[13] *Boston Herald*, December 31, 1909.

Ether.—One other illustration must serve to complete this ungracious paragraph on limitations. I shall not weary you with citations from Lord Kelvin, telling us how much more we know about the ether than about ordinary matter, but I shall trouble you with a single sentence from that skilled expositor, Sir Oliver Lodge,[14] whose latest pronouncement upon this subject, omitting, however, the suppositions with which the entire argument is honeycombed, is as follows:

> The estimates of this book and of "Modern Views of Electricity" are that the ether of space is a continuous, incompressible, stationary fundamental substance or perfect fluid, with what is equivalent to an inertia-coefficient of 10^{13} grams per c.c.: that *matter* is composed of modified and electrified specks or minute structures of ether which are amenable to mechanical as well as electrical force and add to the optical or electric density of the medium: and that elastic rigidity and all potential energy are due to an excessively fine-grained etherial circulation with an intrinsic kinetic energy of the order of 10^{33} ergs per cubic centimeter.

Suffice it to say that I am second to no man in this society in my admiration for that group of men whose names are associated with the following dates—1676, 1728, 1820, 1831, 1845, 1864, 1888, Römer, Bradley, Oersted, Faraday, Neumann, Maxwell, Hertz; names and dates which mark the discovery of the finite speed of light, the discovery of aberration, the discovery of the magnetic field produced by an electric current, the discovery of the electromotive force produced by magnetic displacement, the mathematical formulation of this result by Neumann, the combination of these two results by Maxwell and the prediction from them of electric waves, the experimental realization of these waves by Hertz. For brilliancy of achievement this series has certainly sel-

[14] "Ether of Space," p. 151, Harper, 1909.

dom, if ever, been surpassed in the history of physics.

But leaving matter aside, and considering only the ether, what is the net result? Practically this, that *electromagnetic disturbances, including light waves, are propagated through space with a speed of 300 million meters per second.* This, I conceive to be the criticism which *every* sound metaphysician, but only *some* sound physicists, would pass upon our present knowledge of the ether. This is the one fact concerning the ether which we know in the same sense in which we are said to "know" the ordinary everyday facts of physics.

In conclusion, and still dealing with limitations, I beg to offer for your consideration a definition (*i. e., a* delimitation) of physics recently given to me by an eminent metaphysician.

Last summer I had the pleasure of several times meeting Professor Münsterberg; and on one of these occasions I took the liberty of submitting to him, for criticism, a definition of physics, which I myself had formulated. Following is his definition of the physics of to-day which he, in return, submitted to me and which is, I am inclined to think, unsurpassed in point of accuracy, clearness and completeness:

> Physics deals with changes in the world of over-individual objects, in so far as they are not changes of composition. It consists of those judgments which have proved themselves by trial to determine most accurately our justified expectations concerning these changes. In dealing with *objects* it separates itself from the knowledge of will-acts; in dealing with *over-individual* objects it separates itself from psychology; in abstracting from changes of composition, it separates itself from chemistry. The over-individual objects may be *matter* or *ether* or *electrons*.

CONCLUSION

The view of physics here presented is that of a half truth or partial truth. But this is very far from saying it is an untruth. The essential point—the only essential point—is for us to recognize the facts; to know ourselves; to admit our limitations. Then the more nearly we remain inside these limitations, and avoid "the den of the metaphysician," the better.

That flexibility of mind which it is desirable to secure *by not* translating every temporary opinion into a hard-and-fast fact of nature is well illustrated by a recent remark of Professor Schuster[15] who is himself one of the small group of men who have established the pulse theory of white light. "These two representations of white light (by homogeneous waves and by impulses) are," he says, "not mutually exclusive: They represent two points of view, and we may adopt either one or the other in different problems according to our convenience."

Less fixity and more flexibility in our views concerning the ether might, for instance, permit a more cordial consideration of Professor Osborne Reynolds's theory of gravitation which, so far as I understand it, has much to recommend it.

Lest what I said at the outset concerning the experimental side of physics should be forgotten, let me, in justice to myself, remind you once more of my attitude toward the experimentalist, towards that group which in Italy includes Galileo, Volta, Melloni and Righi; the skillful group which includes Oersted, Kirchhoff, Hertz, Roentgen; the French group of laboratory workers, Mersenne, Fresnel, Regnault, the Curies; in England, Gilbert, Boyle, Joule, Rayleigh; and those dextrous men, our own countrymen, Franklin, Henry, Rowland, Michelson. Toward the experimentalist as compared with the friendly critic and reviewer, my feeling is precisely that of Lincoln toward the soldiers who fought at Gettysburg. You all remember his sen-

[15] *Phil. Mag.*, (6), 18, 767 (1909).

tence—"The world will little note nor long remember what *we say* here; but it can never forget what *they did* here."

Henry Crew
Northwestern University

CHARLES REID BARNES

Charles Reid Barnes was born at Madison, Ind., September 7, 1858, and died at Chicago, Thursday, February 24, 1910. He attended Hanover College, where he graduated with the degree of A.B. in 1877, being the valedictorian of his class. He was a student of Professor Coulter, with whom he was henceforth intimately associated professionally and otherwise until his death. After graduation he studied at Harvard University with Professor Gray, who regarded him as a man of great promise. In 1880 Barnes returned to Hanover College, where he was given the degree A.M. That same year he entered upon an instructorship of natural science at the high school, Lafayette, Ind., and later at Purdue University, where he was promoted to a professorship in 1882. In 1885 his chair was changed from natural science to botany and geology. In the year 1885–6 Professor Barnes again spent some time at Harvard University, and his *alma mater* in 1886 conferred upon him the degree Ph.D. In 1887 he was called to the chair of botany at the University of Wisconsin, whence in 1898 he was called by the University of Chicago to occupy its newly created chair of plant physiology, and here he remained until his death. From 1883 until his death he was associated with Professor Coulter in the editorship of *The Botanical Gazette.*

Professor Barnes was always prominently connected with the various scientific societies, having become a member of the American Association for the Advancement of Science in 1884 and a fellow in 1885. In 1890 he was secretary of the Botanical Club of the American Association for the Advancement of Science, and was secretary of the Botanical Society of America from its inception at Brooklyn from 1894 until 1898. In 1894 he served as secretary of Section G, in 1895 as secretary of the council of the American Association for the Advancement of Science, and in 1896 as general secretary of the American Association for the Advancement of Science. In 1898 he served as vice-president for Section G, American Association for the Advancement of Science, giving his retiring address at Columbus in 1899 on "The Progress and Problems of Plant Physiology." In 1903 he served as president of the Botanical Society of America, giving his retiring address at Philadelphia in 1904 on "The Theory of Respiration." In 1905 Professor Barnes served as a delegate from Section G, American Association for the Advancement of Science, to the international Botanical congress at Vienna. He was also a member of the American Society of Naturalists and of the Botanists of the Central States, and was in turn a member of influence in the state scientific academies of Indiana, Wisconsin and Illinois.

As a botanical contributor Professor Barnes began his career in a modest way in *The Botanical Gazette* in 1877, his first contributions, entitled "Notes," having to do chiefly with annotated lists of plants and additions to county floras, quite in the manner of the time. As early as 1879, however, some of his contributions reveal a strong physiological bent, the necessity of devices for accurate experimentation appealing to him then and ever afterward with unusual force. From 1883, when he became editorially connected with *The Botanical Gazette,* he gave freely of his time and energy to that journal. Much of the remarkable success of this periodical is due to his editorial genius; his trenchant English, and his insistence on accurate statement and mechanical perfection have for many years been reflected on almost every page. Perhaps no botanical reviewer has been so fearless as was Professor Barnes; frank but friendly disapproval of all that seemed bad, whether in fundamental principles, in statement of fact, or in mechanical alignment, was as natural to him as is fulsome praise to most reviewers. Possibly his greatest service to American botany was in his many-sided work on *The Botanical Gazette.*

Professor Barnes was first generally known

to the botanical fraternity through his taxonomic work on mosses, his first publication in this field being an "Analytic Key to the Genera of Mosses," published in 1886 as a bulletin of Purdue University; the following year was published a "Revision of the North American Species of *Fissidens*." In 1890 there was issued by the Wisconsin Academy of Science his "Artificial Keys to the Genera and Species of North American Mosses"; a revision of this work by Barnes and Heald appeared in 1897. There can be no doubt that these keys have greatly stimulated bryological study in this country, because the classic manual of the mosses (Lesquereux and James) is full of difficulties to all but the specialist in the group. A revision of *Dicranum* by Barnes and True practically completed the author's work in this field. While at Chicago Professor Barnes became greatly interested in the special morphological problems presented by the mosses and liverworts, and for several years there was offered in cooperation with Dr. Land a course in the special morphology of the bryophytes. In connection with this work Drs. Barnes and Land made extensive field studies and collections in Mexico in 1906 and 1908. There had already appeared two joint papers, one on "The Origin of Air Chambers" and the other on "The Origin of the Cupule of *Marchantia*"; several other joint papers are in various stages of completion, and are to be issued by the junior author. A general work on the special morphology of the bryophytes had been projected for the immediate future.

In plant physiology Professor Barnes's chief contributions were as a critical reviewer, as a teacher and effective guide in critical research, and as a sponsor for certain points of view. He was early a champion of the restricted use of the term plant food, as opposed to the broader usage, including water and salts. He also advocated long ago the use of the term photosynthesis (or photosyntax) in place of assimilation for the first stages in food-making, and he consistently advocated the restriction of sex terms to sex organs. The vice-presidential address of 1899, and even more the presidential address of 1904, gave stimulating physiological points of view much in advance of current usage. To few is it given to be so effective as a teacher and guide in critical research, and particularly to make clear the actual status of the subject when foggy and uncertain, as is the case in so many divisions of physiology. The botanists whom Professor Barnes has trained will through their teaching and their investigation carry on his ideals and reflect his powerful personality for yet many years. It is a source of intense gratification to his many botanical friends that Professor Barnes was able to revise the final proofs on the physiological part of a general work on botany that is expected soon to appear from the Hull Botanical Laboratory. In this there will be preserved the essence of his physiological point of view and something of his cogent reasoning and lucid style.

In 1898 Professor Barnes issued a botanical text-book for secondary schools, entitled, "Plant Life, Considered with Special Reference to Form and Function." This little book was about the first to deal particularly with physiology and ecology as subjects for study in secondary schools, and found a teaching public unprepared to use it, though the viewpoint here presented now dominates almost everywhere. A briefer edition of this volume, entitled, "Outlines of Plant Life," appeared in 1900.

To his botanical colleagues the death of Professor Barnes seems peculiarly premature, as he died from the effects of an accidental fall in full vigor and health, and just as he was about to round up in monographic form the results of years of study on the bryophytes. It was to have been expected also that before very long he would have incorporated his lectures to advanced classes on "Plant Physics," "Plant Chemics" and "Growth and Movement" into permanent form. To those who knew Professor Barnes intimately it is known that one of the foremost of our botanists has gone, a man great in many lines, and one who, in spite of his frank criticism and pitiless logic, was more than all a friend.

HENRY C. COWLES

A NATIONAL BUREAU OF SEISMOLOGY

The following resolutions were passed by the Seismological Society of America at a meeting held in San Francisco, on March 2:

Whereas earthquakes which are normally recurring phenomena of the earth's crust have in the past caused considerable loss of property and life, and much of the distress and destruction has been due to lack of knowledge of earthquakes and their peculiar mode of action and of proper precautions against injury and,

Whereas the magnitude of the destruction in the Atlantic coastal region (Charleston earthquake of 1886), the Mississippi Valley region (earthquake of 1812) and the Pacific Coastal region (California earthquakes of 1857, 1868, 1872 and 1906), makes it a matter of great import to all the people, and the fact that with increasing density of population the conditions are becoming more favorable for greater destruction in the future makes it important that action be not longer delayed,

Whereas the problems involved are of national and international character and local authority or private enterprise is insufficient to successfully carry on such work, and this fact has been realized already by many of the great nations including Germany, England, Japan, Austro-Hungary, Italy and Spain which have already established scientific earthquake services for the benefit of their peoples and the world at large,

Resolved that the Seismological Society of America strongly favors the establishment of a National Bureau of Seismology with power

(*a*) To collect seismological data,

(*b*) To establish observing stations,

(*c*) To study and investigate special earthquake regions within the national domain,

(*d*) To cooperate with other scientific bodies and organizations and individual scientists in forwarding the development and dissemination of seismological knowledge.

It also favors the organization of this bureau under the Smithsonian Institution with the active cooperation of other scientific departments of the government.

Resolved that copies of these resolutions be transmitted to the President, President of the Senate, the Speaker of the House of Representatives, Secretary of the Smithsonian Institution and the members of the House Committee on Library which has this matter now under consideration.

SCIENTIFIC NOTES AND NEWS

The annual session of the National Academy of Sciences will be held in Washington, D. C., beginning Tuesday, April 19.

The American Philosophical Society will hold a general meeting at Philadelphia on April 21, 22 and 23. On the evening of April 22, there will be a reception in the Hall of the College of Physicians, when Professor George E. Hale will deliver an illustrated lecture on "The Work of the Mt. Wilson Solar Observatory." The annual dinner will be held on the evening of April 23.

Professor Giovanni Virginio Schiaparelli, the eminent Italian astronomer, has celebrated his seventy-fifth birthday.

Dr. Wilhelm Hittorf, professor of physics at Münster, has been elected a member of the Paris Academy of Sciences.

Professor W. M. Davis, of Harvard University, has been elected an honorary member of the Società Geografica Italiana in Rome.

Dr. W. J. Holland, director of the Carnegie Museum, has been elected a corresponding member of the Royal Academy of Sciences, at Bologna, to fill the vacancy created by the death of Albert Gaudry, of Paris.

Dr. A. Hrdlička, of the U. S. National Museum, has been made a corresponding member of the Anthropological Society in Vienna.

Sir William Ramsay has been nominated as honorary member of the Chemical Society of France.

Sir Thomas Barlow, F.R.S., has been elected president of the Royal College of Physicians, London, in succession to Sir Richard D. Powell.

Dr. F. W. Putnam, honorary curator of the Peabody Museum of American Archeology and Ethnology, Harvard University; Dr. R. B. Dixon, assistant professor of anthropology and Dr. A. M. Tozzer, instructor in Central American archeology, have been appointed delegates of Harvard University at the foundation of the Mexican National University in September, 1910. Professor Dixon has also been appointed delegate at the Inter-

national Congress of Americanists to be held at the City of Mexico at the same time.

MR. H. H. CLAYTON, late of the Blue Hill Observatory, has gone to Buenos Ayres to organize kite and balloon observations under the direction of the Argentine Meteorological Service.

DR. SEBASTIAN ALBRECHT, of the Lick Observatory, has been appointed first astronomer in the National Observatory of the Argentine Republic.

THE annual address before the Huxley Society in the Johns Hopkins University was delivered Friday evening, April 1, by Professor W. P. Montague, of Columbia University. The address was on "Life and Mind as Forms of Energy."

PROFESSOR A. E. KENNELLY, of Harvard University, gave a lecture on March 12, to graduate students of the U. S. Naval Academy at Annapolis, on "The Operation of Electric Motors from a Central Power Station."

THE Aldred lecture of the Royal Society of Arts will be delivered by Professor H. H. Turner, F.R.S., on May 4, the subject being "Halley and his Comet."

MR. T. A. RICKARD, editor of the *Mining Magazine,* London, has been appointed lecturer on mining geology at Harvard University, where he will deliver a course of lectures at some time during the present year.

COMMITTEES of members and friends of Glasgow University have, says *Nature,* procured contributions to some £1500 for the purpose of commemorating the services of Dr. John Cleland, regius professor of anatomy from 1877 to 1909, and Dr. William Jack, professor of mathematics from 1879 to 1909, who retired last year. It has been decided to present to the university a portrait of Dr. Cleland, painted by Sir George Reid, with a replica for Mrs. Cleland; and a portrait of Dr. Jack, painted by Sir James Guthrie, and also a prize, to be awarded at intervals, for the best thesis on a mathematical subject approved for the degree of doctor of science during the preceding period.

IT is proposed to erect at Marburgh a monument in memory of Wilhelm Roser, who held the chair of surgery in the University of Marburg from 1850 to 1858.

MR. SAMUEL WARD LOPER, curator of the Museum of Wesleyan University, the author of contributions to geology and paleontology, has died at the age of seventy-five years.

MR. J. RAYNER EDMANDS, assistant in the Harvard College Observatory, died on March 26, at the age of sixty years.

THE death is announced of Dr. Eduard Pflüger, the eminent physiologist of Bonn, founder and editor of Pflüger's *Archiv.*

DR. OTTO HERMES, first director of the Berlin Aquarium, has died at the age of seventy-one years.

MR. CHARLES FOX-STRANGWAYS, for many years connected with the British Geological Survey, died on March 6, at the age of sixty-six years.

THE Central Branch of the American Society of Zoologists will hold its annual meeting at the University of Iowa on April 7, 8 and 9. The address of the president, Professor E. A. Birge, of the University of Wisconsin, is entitled "Some Personal Peculiarities of Lakes."

THE Association of German Scientific Men and Physicians and Medical Practitioners will hold its eighty-second meeting at Königsberg this year from September 18 to 24.

THE bequest of Miss Phebe Anna Thorne to the American Museum of Natural History has been applied as an endowment to the museum's room for the blind. Messrs. Samuel and Jonathan Thorne, the executors of the will, have increased the amount from ten thousand to twenty-five thousand dollars.

THE following course of illustrated lectures in economic entomology and genetics is to be given at the Bussey Institution of Harvard University, Forest Hills, on Sunday afternoons, during April and May, at 4 o'clock:

April 10—"Insects as Carriers of Disease. I. The House-fly and its Allies," by Professor W. M. Wheeler.

April 17—"Insects as Carriers of Disease. II.

Mosquitoes and their Allies," by Professor W. M. Wheeler.

April 24—"Mendel's Law of Heredity," by Professor W. E. Castle.

May 1—"Variation and Selection in Evolution and in Animal Breeding," by Professor W. E. Castle.

May 8—"The Gypsy and Brown-tail Moths," by Mr. C. T. Brues.

May 15—"Insects Injurious to Elm Trees," by Mr. C. T. Brues.

May 22—"Making New Plants by Selection," by Professor E. M. East.

May 29—"Making New Plants by Hybridization," by Professor E. M. East.

The Eastern Branch of the American Society of Zoologists at its recent meeting in Boston designated the following persons as a committee to cooperate with the commission on nomenclature of the International Zoological Congress: Dr. H. B. Bigelow, Museum of Comparative Zoology, of Harvard University, *chairman;* Dr. A. Petrunkevitch, American Museum of Natural History; Professor J. S. Kingsley, Tufts College; Dr. A. G. Mayer, Carnegie Institution of Washington; Dr. J. P. Moore, Philadelphia Academy of Natural Sciences.

One of the field courses announced in the Harvard Summer School is a physiographic excursion to be conducted by Professor W. M. Davis in the Rocky Mountains of Colorado during three weeks in July, beginning at Denver, July 6. The object of the trip is to study various features of mountain form, with special attention to the best method of describing them. The points to be visited are: the normal and glacial features of the highlands and continental divide of the Front range at the head of Boulder creek, crossed by rail at an altitude of 11,680 feet; the foot hills in the neighborhood of Golden and the valley of Clear Creek; the foot hills in the Garden of the Gods near Colorado Springs, and the fault-line escarpment of the Front range next to the south; the highlands west of Colorado Springs, over which Pikes Peak rises like a great monadnock; South Park, as an example of a high-level intermont basin; the upper Arkansas valley, as an example of a deeper intermont basin; the normal and glacial features of the Sawatch range west of the Arkansas valley; the Royal gorge of the Arkansas in the Front range, and the foot hills near Canyon city; the lava-capped Raton mesa near Trinidad; and the district of dike-walls on the denuded slopes of the deeply dissected ancient volcanoes, known as Spanish peaks. The conditions on which students (men only) may join the party can be learned on addressing Professor Davis, at Cambridge, Mass. The party will travel from place to place by train or wagon, making short distances on foot, and stopping in hotels over night with possibly one or two nights in camp.

Letters have been received at the Harvard College Observatory from Professor E. B. Frost, director of the Yerkes Observatory, giving the following observations by Professor Bernard: Comet *a* 1910 was observed March 12, 1910, at $16^h\,56^m$ central standard time, in R. A. $22^h\,24^m\,39^s$ and Dec. $+15°\,37'.3$ (1910.0). "The comet was of the ninth magnitude, strongly condensed, possibly to a very faint nucleus. No tail noticed." A photograph of the same comet was obtained at dawn on March 14. No tail was shown on the plate with an exposure of nine minutes. Photographs made with all three lenses of the Bruce telescope, the exposure being $1^h\,50^m$, failed to show any trace of the comet reported by Pidoux. These plates show a tail to Halley's comet nearly a degree long. A photograph of Comet *a* 1910 at dawn on the morning of March 15, the exposure being 35^m, shows a faint tail two degrees long.

The North Dakota Geological Survey has recently published the fifth volume of its series of reports dealing with the geology and natural resources of the state. The present report, which is a volume of 278 pages, with many illustrations and maps, contains papers on the geology, topography and coal deposits of southwestern North Dakota, including the Little Missouri badlands; the geology of the northeastern portion of the state with particular reference to the natural cement rock of that region; together with chapters on the

geology of North Dakota as a whole, on natural gas and on good roads. Previous reports of the Survey have described the extensive and valuable clays and lignites of the state, the fourth report being devoted entirely to the clays, particularly the high grade fire and pottery clays. Last summer the geology and natural resources of the area comprised in the Bismarck quadrangle was investigated by the State Geological Survey in cooperation with the United States Geological Survey. These two surveys are also cooperating in the collection of data regarding the deep wells of the state, and this work will be pushed as rapidly as possible. During the coming summer the study and mapping of the geological formations of south-central North Dakota will be continued, and work will also be undertaken on the physiography and geology of the interesting Devils Lake region.

IT is stated in *Nature* that the director of the British Meteorological Office has given notice that from April 1 forecasts of the weather prospects more than twenty-four hours ahead will be issued as opportunity is afforded. Applications have been received at the Meteorological Office from time to time for forecasts of weather several days in advance, in addition to, or instead of, the usual forecasts which refer to the twenty-four hours reckoned from the noon or midnight following the issue of the forecasts. According to the experience of the Meteorological office, the weather conditions do not usually justify a forecast detailing the changes of weather for consecutive days. There are a number of occasions in the course of the year when the distribution of pressure is typical of settled weather, and also occasions when the conditions are characteristic of continued unsettled weather. On these occasions, and on a few others when the sequence of the weather is of a recognized type, a sentence giving in general terms the outlook beyond the twenty-four hours of the definite forecast might be useful to the general public, and, as it could be justified by the statement of definite reasons for the inference, it would come within the general rules laid down by the office with reference to the issue of forecasts. An indication of the general prospect extending beyond the twenty-four hours' limit is frequently given in the "General Inference" which precedes the forecasts for the several districts on the sheet issued to newspapers. It is expressed in more or less technical language, and the application to the several districts might only be followed by persons acquainted with the terminology used in weather study. It is proposed, therefore, when the meteorological conditions permit, to supplement the forecasts for districts by a remark on the further outlook.

UNIVERSITY AND EDUCATIONAL NEWS

AMONG recent endowments to the New York Polyclinic Medical School and Hospital, the first post-graduate medical school in the United States, are one of $250,000 by Mr. William P. Clyde, and another of $125,000 by Mrs. Helen Hartley Jenkins.

MR. ANDREW CARNEGIE has given $40,000 to Wells College for the building of a library to be called the Frances Cleveland Library in honor of Mrs. Grover Cleveland, who is a graduate of the college.

THE main building of the Texas Christian University has been destroyed by fire, entailing a loss of $125,000.

PRESIDENT NOBLE has issued a formal announcement of the fact that, by order of the board of trustees and by act of the General Assembly of Maryland, the corporate name of the Woman's College of Baltimore has been changed to Goucher College.

IN the Medical School of the University of Pennsylvania Dr. Alonzo E. Taylor, now of the University of California, will become professor of physiological chemistry; Dr. Henry T. Ricketts, of Chicago University, will occupy the chair of pathology, and Dr. Richard M. Pearce, of Albany, will be professor of experimental medicine. Dr. Allen J. Smith, while retaining his position as dean of the school, will be transferred to a chair of comparative pathology and be at the head of the courses in tropical medicine.

Professor Samuel C. Prescott has been appointed acting head of the department of biology of the Massachusetts Institute of Technology, during the absence in Europe of Professor W. T. Sedgwick.

Dr. E. H. Cameron, instructor in psychology in Yale University, has been advanced to the grade of assistant professor. In that institution Dr. F. S. Breed, now engaged in graduate work in comparative psychology at Harvard University, has been appointed instructor in psychology.

Mr. Alan S. Hawksworth has been appointed professor of higher mathematics in the University of Pittsburgh.

At Haverford College Professor A. H. Wilson, of the Alabama Polytechnic Institute, has been appointed to the position of associate professor of mathematics in place of Professor Jackson, who returns to England.

Willis T. Pope, professor of botany in the College of Hawaii, has been appointed by the governor, superintendent of public instruction for Hawaii. Vaughan MacCaughey (Cornell, '08), has been appointed to fill the vacancy in the college.

DISCUSSION AND CORRESPONDENCE

SOME ADDITIONAL CONSIDERATIONS AS TO THE CARNEGIE FOUNDATION

To the Editor of Science: Several contributors to your journal have recently discussed the change of policy announced by the Carnegie Foundation; two considerations, however, have not been mentioned either here or elsewhere to my knowledge.

First, the obligations on the part of the foundation toward those formerly denominational colleges which have in the last four years secured changes in their charters severing their relations with the parent denomination. The reports of the foundation have mentioned several of these institutions, and others have come within my notice. In all these cases, the foundation held out to these institutions the promise of certain benefits if they would sacrifice the historic association with the people who founded the school. These benefits were essentially two,—the privilege to professors of retiring after twenty-five years of service, and of retiring on a somewhat higher pension at the age of sixty-five. Now, in the present situation, these colleges find themselves left with only a small fraction of the benefit anticipated, for nobody will deny that the service pension was a much greater inducement than the age pension. And the most disquieting thing about it is that this great foundation in no way intimates a consciousness of having treated anybody unjustly.

Second, as to the state universities. If the service pension be discontinued, has the foundation anything to offer to the professor in such an institution? Is there a state university in the land where a professor sixty-five years of age with a fifteen-year (and generally a thirty-year) record in the institution behind him is in danger of losing his position? I think not. On the contrary, my impression is that the old professors are universally held in such respect, and their lives are so interwoven with the history of the school, that no one thinks of dismissing them in their old age. Possibly in some small and poor private colleges of the country the condition of the exchequer may make it hard to do justice to old professors, but no state university can afford to deal otherwise than generously with such cases. But what will the foundation do for them when they reach the age of sixty-five? It will "automatically, and as a matter of right, and not as a charity," reduce their salaries about fifty per cent.! As an offset to this, there is the possibility of a disability pension, and the probability of a pension to the widow of a professor. It would take considerable actuarial ability to figure out whether the professor and his wife are ahead or behind when both sides are considered. It is easy to see that the foundation has virtually made a contribution to the treasury of the university, but has it on the whole done anything to compensate the professor for the privations of a life time of poorly paid service, as so generously desired by Mr. Carnegie when he made his first gift to the foundation?

J. M. Aldrich
University of Idaho,
March 30, 1910

[The Carnegie Foundation for the Advancement of Teaching is of such importance for education and science that we should be pleased to see all aspects of the subject thoroughly discussed in this journal. As the communications hitherto received have been critical, we should like to have letters emphasizing the services of the foundation and defending the recent action of the trustees.—ED.]

KAHLENBERG'S CHEMISTRY

TO THE EDITOR OF SCIENCE: "The penalty of being oracular is that fashions in oracles change." This clipping from a daily paper was called to mind by reading Lewis's recent review of Kahlenberg's excellent text. In this review, one whose experience is slight in teaching the first-year student gives us exact advice as to what the beginner should be taught.

Among chemical circles, the first-year course stands much as Walker used to describe the position of political economy among popular sciences. Every man thinks he is capable of taking part in a subject of such general interest. The citadel has been assailed by every new fad in chemistry until it is a by-word that, compared with mathematics and the classics, chemistry stands out prominently characterized by the unsettled conditions of its pedagogical method.

While admitting the greatest appreciation of the value of those topics for which Lewis argues so ably (as though physical chemistry needed to be propagated and popularized) the question which is most important and which the reviewer does not discuss is the suitability of these topics for first-year students. This is, I imagine, clearly answered by the fact that by far the larger number of college teachers, after studying the presentation of these topics, are not including them in the first-year course. And this is not through ignorance, as Lewis implies, but through judgment born of experience with first-year students. The chemistry of a "generation or more ago" still lives and is ready to say to its youngest branch that it does not pay to rail at one "who has the age on you."

It is unfortunate that the reviewer, because he must ride his hobby and perhaps because he feels that the confidence which he formerly had in the ionic hypothesis has been somewhat weakened by this same Kahlenberg, should have forgotten to point out how excellently each chapter in the text under discussion is presented—how Kahlenberg's rich experience has brought him close to a knowledge of just what the beginner wants to know in the way he wants to have it presented—the beautifully balanced thoughts, the logical sequence. I have just finished reading the chapter on Sulphur. In my opinion, those of us who are teachers and are not afraid to introduce as much of the ionic hypothesis as our pupils need will have already decided with the writer that we have here the work of a master in the good old art of teaching.

The question of what may and what may not most suitably be provided for the beginner should be left for discussion to the section of chemical education; but if I may be allowed to restate from a recent address at Ann Arbor, it is not a question, in the first year, as to what we think it would be desirable for all students of chemistry to know. It is rather the "care and feeding of children" which is thrust upon us for discussion. It is perhaps because we do our work so well, concealing the difficulties, that the teachers of advanced work and the specialist think we can impose anything upon the students and succeed.

In conclusion, would it not be better if the task of reviewing a work which stands for years of enthusiastic interest and successful experience among beginners should be given to one whose interest, as expressed in the review, is sympathetic with pedagogical problems?

ARTHUR JOHN HOPKINS
AMHERST COLLEGE

BOTANICAL-EDUCATIONAL INFORMATION WANTED

TO THE EDITOR OF SCIENCE: In connection with certain important committee work for the Botanical Society of America, I need to know exactly which universities, colleges and technical schools in this country accept the College Entrance Examination Board's certificates for examinations passed upon its one-year unit (or course) in botany, counting

about one point out of fourteen for admission. My present data, derived from official sources, here follow, but they are, for sundry reasons, incomplete. I wish to request that any reader of this note who is connected with a university, college or technical school, will make sure whether his institution is correctly represented in the lists below, and if not I shall be very grateful if he will communicate to me the suitable correction. I shall later publish a supplementary list, and finally a complete one in connection with other related data.

The following institutions accept the College Entrance Examination Board's examinations in botany, and state the fact in their official publications: Bryn Mawr, California, Cincinnati, Columbia, Cornell, Dartmouth, Harvard (although it can count for only a half year), Illinois, Leland Stanford, Maine, Massachusetts Institute of Technology, Massachusetts Agricultural College, Mount Holyoke, Nebraska, Northwestern, Ohio, Pennsylvania, Rochester, Simmons, Smith, Syracuse, Washington (St. Louis), Wellesley, Wells, Vermont, Woman's College of Baltimore, Yale Scientific School.

The following institutions, I am assured, accept the board's examinations, although at last accounts no mention of the fact had been made in their official publications: Chicago, Haverford, Kansas, Minnesota, Missouri, North Carolina, Oberlin, Wabash, Williams.

W. F. Ganong

Northampton, Mass.

SCIENTIFIC JOURNALS AND ARTICLES

The contents of the March issue of the *Journal of Terrestrial Magnetism and Atmospheric Electrictiy* are as follows: "Scientific Staff and Crew of the *Carnegie* at Falmouth, England, October, 1909" (Frontispiece); "Completion of the First Cruise of the *Carnegie*"; "The Present State of Our Knowledge of Magnetic Materials," A. A. Knowlton; "Beginning and Propagation of the Magnetic Disturbance of May 8, 1902, and of Some Other Magnetic Storms," L. A. Bauer; "Analysis of the Magnetic Disturbance of January 26, 1903, and General Considerations Regarding Magnetic Changes," L. A. Bauer; "The Magnetic Storm of September 25, 1909, at de Bilt, near Utrecht, Holland," G. van Dyk; "Discontinuance of the Baldwin Magnetic Observatory and Establishment of the Tucson Magnetic Observatory," R. L. Faris; "Principal Magnetic Storms Recorded at the Cheltenham Magnetic Observatory, October–December, 1909," O. H. Tittmann; "Aurora Borealis observed at Beinn Bhreagh, near Baddeck, Nova Scotia, September 21 and October 18, 1909," A. G. Bell; "Magnetic and Allied Observations in connection with Halley's Comet"; "Hellmann's Bibliography of Magnetic Charts," L. A. B.; "Galitzin, Arnold, The Beginning of an Earthquake Disturbance," H. F. Reid; "The Tenth Edition of Müller-Poullet's Physics (Vol. IV., Pt. 1)," W. G. Cady.

SCIENTIFIC BOOKS

Radiation, Light and Illumination. A Series of Engineering Lectures Delivered at Union College. By Charles Proteus Steinmetz, A.M., Ph.D. Compiled and edited by Joseph Le Roy Hayden. Pp. xii + 305. New York, McGraw-Hill Book Company. 1909.

This latest book from the pen of Dr. Steinmetz constitutes to some extent a departure from his previous writings. In it an attempt, perhaps the first definite attempt, has been made to bring together not only the principal physical facts, but also many of the more important physiological facts which pertain to the effects of luminous and attendant radiation. The view-point throughout is that of the engineer. The book is the outcome of a series of lectures to engineering students. It is intended in the author's words in the preface "not merely as a text-book of illuminating engineering, nor as a text-book on the physics of light and radiation, but rather as an exposition, to some extent, from the engineering point of view, of that knowledge of light and radiation which every educated man should possess, the engineer as well as the physician or the user of light."

With the exception of a few chapters there

is no mathematics, and throughout there is evidence of a desire to make the book attractive to the non-technical reader. It is somewhat doubtful, however, whether the book will appeal to many except students or technical men who desire a brief but comprehensive survey of certain phases of illuminating engineering. As a whole the book is suggestive, and should be of distinct value in helping to correlate the various phenomena of physics and physiology on which the scientific side of illuminating engineering rests.

It would scarcely seem, however, that a text-book for students, or an exposition for the general educated public, should be the proper place to introduce new ideas and terms which have not yet been accorded general acceptance by scientific writers, and yet the present book contains many such innovations. Wave-lengths are expressed in micro-centimeters (cm. $\times 10^{-6}$) on the ground that there are several other systems in use, none of which is scientifically accurate (p. 7) according to the C.G.S. system; and yet several pages further on (p. 16) a sudden jump is made from centimeters to feet. The classification (p. 20) of "the total range of known radiations" into "the *electric* waves and the *light* waves" would scarcely seem orthodox or clarifying, particularly as the "light" waves are made to include even X-rays.

In Lecture IX. a number of types of photometers, some of them quite primitive, are described, and several pages are given to a description of a so-called "luminometer" which employs the old but sometimes very useful method of "reading distances," whereas no mention has been found of one of the most common, and perhaps the most accurate photometer in use at the present day for comparing lights of approximately the same color—the Lummer-Brodhun photometer in its two forms. Even the very familiar Bunsen photometer, though mentioned by *name,* is nowhere described. The photometer shown in the diagrammatic sketch and described under the name "Bunsen" on page 170 is in reality a simple Ritchie wedge, distinctly different from the "grease spot" photometer invented by Bunsen, or even the more recently improved Leeson disk which is sometimes substituted for it. Again (p. 260) it would seem that too little weight is given to the accepted definition of "illumination" compared with the author's idea of what this term should indicate.

Lecture III., Physiological Effects of Radiation, would seem to the reviewer to be very unfortunate in its manner of presentation. As the present knowledge in this field, particularly in regard to the "pathological and other effects on the eye," is quite restricted, and only to a very limited extent satisfactorily established, one is likely to wonder whether the many positive statements are correct expressions of accepted facts or merely speculation. Here, particularly, a few references to authoritative sources of information would be appreciated. It is perhaps questionable whether the various harmful effects of light on the eye can be so readily classified into the two distinct groups, "power effects" and "specific effects of the shorter waves." It is quite probable that a definite large amount of radiant energy incident on the eye would be capable of producing entirely different results if all of the energy were in the infra-red, or if all were concentrated in the most luminous portion of the visible spectrum. We can look at an incandescent mantle or an incandescent filament for a brief period without any pronounced feeling of pain, but what would the result be if all of the radiant energy from these sources could be transformed into light, even of the longer wave-lengths where the so-called "specific effects" presumably do not enter? The dazzling glare in such an experiment, were it practicable, would very probably be distinctly painful. In general it is necessary to consider the quality in conjunction with the quantity, so that the classification suggested would scarcely seem justified. Moreover, it would seem wise to discriminate between those effects which pertain to the anterior portions of the eye as in the absorption of large quantities of ultra-violet radiation, and those harmful effects which are retinal.

Lecture V., Temperature Radiation, gives a very brief résumé (with a new notation) of the laws of temperature radiation. In the generality of the statements, however, accuracy is ofttimes overlooked. It is not true that (p. 74) "Practically all bodies give the same temperature radiation, *i. e.*, follow the temperature law (1)" (which states that the total emissivity is proportional to the *fourth* power of the absolute temperature). For most substances investigated the exponent should be greater than "4," in some cases (see recent investigation by Coblentz, Bureau of Standards Bulletin, Vol. 5, p. 339) as large as 6 or even 7. Little attention is given to the effect of selectivity (though slight mention is made of it) in determining the high efficiencies of some sources, such, for example, as the osmium lamp. It is at least questionable whether (p. 80) the melting point of osmium is higher than that of tantalum merely because it can be operated at a higher efficiency. Osmium undoubtedly is distinctly selective.

On the whole, although the book is extremely interesting to the technical reader and is quite suggestive, there would appear to be a lack of care in gathering together the facts, and a somewhat too dogmatic style in presenting those topics which are still more or less in the domain of speculation. The color pyrometer described on pp. 89–90 is apparently a real instrument, but any attempt to reproduce it would soon convince one that no mixture of spectrum yellow and spectrum blue would give a green that could be matched in hue with spectrum green. Numerous small errors, both typographical and factual, could be cited, but would scarcely strengthen the conclusion that an early revision of the book would be most welcome.

The reviewer desires, however, to express his appreciation of the service which this book has rendered in coordinating the closely related phenomena of physics and physiology in their relation to illumination, and in calling attention to many vital questions of illumination which are frequently given too little attention in practise (such as those of directed and diffused illumination, shadows, the effects of sources of high intrinsic brightness in the field of view, etc.).

Edw. P. Hyde
Physical Laboratory,
National Electric Lamp Association,
Cleveland, Ohio

Die Säugetierontogenese in ihrer Bedeutung für die Phylogenie der Wirbeltiere. Von A. A. W. Hubrecht. Jena, Gustav Fischer. 1909. Pp. 247, 186 text figures.

Most zoologists know that Professor Hubrecht has been an assiduous student of mammalian embryology for many years. The reviewer well remembers the beautiful preparations—probably of *Tupaja*—exhibited by the author at the Oxford meeting of the British Association in 1895.

The appearance of a volume on the subject from such experienced hands may be supposed to be an occurrence of no little interest to students and teachers in this province of biology. Whether the volume that actually comes to us fulfills expectations depends largely on what the particular user may feel in need of, and what his standpoint may be with reference to the more general problems involved.

If one be chiefly desirous of a manual that should set forth the main facts of mammalian development positively ascertained up to the present moment, along with such generalizations as a conservative zoologist might recognize as truly illuminating and not objectionably forced, the book can not be very satisfactory, so it would seem.

If, on the other hand, one would wish to see how strong a case a competent specialist can make of a fundamental theory of his own, then the work may be adjudged satisfactory. What we have essentially is a case of special pleading, as indeed the title permits if it does not intend us to infer. Not mammalian ontogeny, but such ontogeny in its significance for vertebrate phylogeny, is the aim.

This statement is not intended to give the impression that the reader longing for facts primarily will find nothing to his purpose. Not only the text but the many figures present very many facts. Such a summary, for ex-

ample, as is given on page 3, of the chief works on the cleavage of mammalian eggs, should be highly appreciated by the general student.

There are in all six chapters, as follows: I., The First Cell Layers—(A) Of the Monodelphic and Didelphic Mammals; (B) Of the Ornithodelphic Mammals and Sauropsida, and (C) Of the Ichthyopsida. II., Farther Development of the two Germ-layers of the Vertebrata up to the Origin of the Somites. The Mammalia, the Amphibia, the Sauropsida and Ornithodelphia and the Fishes are treated. III., Diplotrophoblast-Serosa (Subzonal) Membrane, Chorion, Allantois and "Nabelblase" in Onto- and Phylogenesis. IV., The part taken by the Trophoblast in the Nutrition and the Attachment of the Embryo. V., Various Points (Verschiedenes) on Placentation. VI., Considerations Touching the Phylogeny and the Systematic Divisions of the Vertebrata.

These contents of the chapters will suffice to show that as regards embryology proper only the very early stages are dealt with. Organogenesis does not fall within the scope of the work. The undertaking is such, too, that vertebrates other than mammals receive large attention. Of the 186 figures nearly one fourth are not mammalian, the larger number of these outsiders being of fishes, amphibians and reptiles.

As to exactly how much weight should be attached to Hubrecht's theory in its various ramifications (his trophoblast theory) only a student of the vertebrata can tell who is more experienced than he, and is far less of a special pleader. But any zoologist who is moderately well informed first hand in general vertebrate morphology and embryology, and who has likewise occupied himself in a serious way with problems of phylogeny, can readily see that the best that can be said for the most far-reaching contentions is that they may *possibly* be true. While it may be legitimate for a zoologist to find a measure of satisfaction in recognizing the various possibilities as to what the course, or rather courses, of vertebrate evolution may have been, it is well never to lose sight of the fact that what is only possibly true is probably not true.

Hubrecht has pointed out facts enough to make it *possible* that his "vermactinial stage in vertebrate phylogenesis," figured on page 22 and again on page 228, was a reality in some remotely past time. But dozens of other facts which he has not alluded to make it *probable,* to the reviewer's mind at least, that no such ancestral stage ever did exist.

Such a hypothetical creature would be harmless, indeed might have a certain usefulness, could it be presented merely as one among numerous possibilities, for if so presented it would not be chargeable, as it is almost sure to be when claiming exclusive rights, with distorting the facts upon which its existence depends; and it, along with its alternatives, might then help the mind to grasp the general truth that the actual animals dealt with have arisen by a natural, that is, an evolutionary process.

LA JOLLA, CAL. WM. E. RITTER

THE SILKEN-HAIRED ONES

WHAT "Black Beauty" did for horses President Jordan's "Story of Matka"[1] ought to do for the unfriended fur-seals of the Bering Sea. The ruthless slaughter of these seals which will end, if not soon interrupted, in their certain extinction, is a hideous present-day world crime of which three great powers are openly guilty. In 1880 two and a half million fur-seals lived in Bering Sea. In this year 1910 of enlightened civilization, scientific knowledge and Christian sweetness and light there are still by good fortune alive 150,000 of these beautiful, silken-haired, soft-eyed creatures of nature's choicest making. The others have been slaughtered as mothers or starved as children by the refined methods of diplomacy cultivated by Great Britain, Japan and the United States.

Dr. Jordan wrote the "Story of Matka" on the very rocks where Matka lived, with Matka the mother seal and Kotik, the baby seal and Atagh, the grandfather and Eichkao, the blue

[1] "The Story of Matka," by David Starr Jordan. San Francisco, Whitaker-ay-Wiggin Co., 1910.

fox that "clin-n-g, clin-n-g-ed like a scared buzz-saw," and wise old Eparka, the sea-parrot, all under his keen eyes. He was there as the special correspondent of the great newspaper "Beneficent Science" which is published for the amusing, informing and guiding of all the men and women of the world. But the story moved no man nor woman; that is moved none to action. Or rather it did not move the needed many to compel the action that is necessary if the few Matkas and Kotiks that are left are not to be the last of their kind.

So now the story is reprinted in such form that it is to be offered to the children of the land to see if perhaps they may not feel more and do more than their fathers. It is a well-illustrated, simply told true tale, at once charming and pathetic, fascinating in its revelation of the wonderful ways of a child-bearing and child-teaching wild animal of the mist-wrapped islands of the north, appealing to every chord of sympathy and rousing to every instinct of antagonism for brutal cruelty.

V. L. K.

Laboratory Manual of First Year Science for Secondary Schools. By Russell and Kelly. New York, Henry Holt & Co. 1909. Pp. 163.

This book gives the first printed account of the rather famous Springfield course in general science introduced five years ago by Dr. Thomas M. Balliet and Wm. Orr, then respectively superintendent and principal of high school, at Springfield, Mass.; now, respectively, dean of the school of pedagogy, New York University, and deputy commissioner of education of the state of Massachusetts. The authors have developed this course in great fidelity to the ideas and suggestions of their superior officers.

The purpose of the course is twofold: "(1) To give the pupil a broad general view of the whole field of science, (2) to explain to the pupil his every-day environment."

The work as it is conducted in Springfield is in a large measure informational, with abundant experimental illustration to make the knowledge *real.* The lecture by the teacher, and the investigation by the pupils of matters to be found outside of the school, are the most effective features of the course.

The course is required of all first-year high-school students (those who have visited the school know that it would be easier to require than to prevent their taking it).

The course is flexible and changes from year to year and is, after all, a "*method of instruction*" rather than a "*course of study.*" Such topics are treated as the following:

Reading of gas meters, water meters, electric meters, reading of water pressure and steam pressure gauges, water tests, charcoal filters, litmus tests, removal of stains, coal tar dyes, food tests, heating and ventilation, uses of the electric magnet, constellations, standard time, weather reports, candle power of light, cost of lights, germination of seeds, leaves, mould, building stones, ores.

John F. Woodhull
Columbia University

SPECIAL ARTICLES

PULSATIONS IN SCYPHOMEDUSÆ DEPRIVED OF THEIR MARGINAL ORGANS

While working at the Harpswell Laboratory, I found that the two Scyphozoa so common on the coast of Maine, *Aurelia flavidula* and *Cyanea arctica,* responded differently to operations on the marginal organs. The European species of these two genera have been studied by Eimer and Romanes, with respect to this point and these two men were unable to agree as to the behavior of the animals with excised marginal organs. Mayer has stated that *Aurelia* (he does not specify the species) is temporarily paralyzed when the marginal organs are excised and this agrees with my observations. Eimer, too, reached a similar conclusion, but Romanes's experiments led him to state that, while many specimens did regain their pulsations, although always irregular and obviously different from those of a normal specimen, yet the greater majority remained quiescent.

Romanes failed to be as explicit in his statements concerning the behavior of these

jelly-fishes as he has in his other writings, and it is not a matter of surprise that he should be quoted[1] as saying that the forms with which he worked became paralyzed, when the marginal organs were excised, when one reads[2] that he found "in all the species I have come across that excision of the margins of the umbrellas produces an effect analogous to that which is produced by excision of the margins of the Hydromedusæ" where such an operation results in the total paralysis of the bell. However, when one reads farther, he says, with much verbosity, that

> There is an important difference, however, between the two cases in that the paralyzing effect of the operation on the umbrellas (of the Scyphomedusans) is neither so certain nor so complete as it is on swimming bells (of hydromedusæ). That is to say, although in the majority of experiments such mutilation of umbrellas is followed by immediate paralysis, this is not invariably the case.

Romanes found that *Aurelia aurita* showed "instantaneous and complete paralysis of the gonocalyx" on excision of the marginal organs, while *Cyanea capillata* was less marked in this respect.[3] Eimer's observations were practically the reverse of this.

There can be no question that Romanes was entirely correct in his observations, for he repeated them during several summers, specifically examining the point in question in the light of Eimer's work. It is fair to assume, too, that Eimer made no mistake. Hence, it seems that *Aurelia aurita* reacted differently on Cromarty Firth, Scotland, from what it did in the North Sea with respect to the matter at issue. Romanes probably used a different species of *Cyanea* (*Cyanea capillata*) from Eimer's form (which was probably *Cyanea lamarckii*) and I have used a third species, *Cyanea arctica*, which seems to be recognized by systematists as a good species. My species of *Aurelia*, *Aurelia flavidula*, is recognized by some as distinct from *Aurelia aurita*, but both Louis and Alexander Agassiz did not so regard it.

From my observations, *Aurelia flavidula* very rarely is paralyzed completely and, indeed, I have but an impression that I have seen *Aurelia* absolutely quiescent after the marginal organs have been removed. Unfortunately, I did not examine the question critically until last year and my previous observations were not recorded. During the past year, however, I found no specimen which did not regain pulsation after a longer or shorter period after the marginal organs were removed. The case of *Cyanea* is directly the reverse, for this form becomes totally paralyzed when the organs are removed. Reference to the statements from Eimer and Romanes, given above will make it clear how these observations correspond to theirs. They agree closely with those of the former and are totally at variance with those of the latter.

The matter is of importance from the point of view of the physiologist who wishes to use some primitive form of contractile substance with which to experiment and these observations are especially directed to them. *Cyanea arctica* will remain quiescent after the marginal organs are removed and respond only to mechanical, chemical and other external stimuli supplied by the operator. In fact, *Cyanea* rivals the classic *Cassiopea* for experimental work. *Dactylometra* reacts like *Aurelia flavidula*.

MAX MORSE

NEW YORK,
March 8, 1910

[1] For instance, Parker in his *Popular Science Monthly* articles on the nervous system makes such a statement and while giving no references, yet he has written me that he was impressed that Romanes's observations led to such conclusions.

[2] "Jelly-fishes, Starfishes and Sea Urchins," Appleton.

[3] *Phil. Trans.*, Vol. 167.

THE SOCIETY OF AMERICAN BACTERIOLOGISTS

THE eleventh annual meeting of the Society of American Bacteriologists took place on December 28, 29 and 30, 1909, in the administration building of the Harvard Medical School, Boston, in conjunction with the annual meeting of the American Association for the Advancement of Science. It can be confidently asserted that the society has never held a more successful and profitable meet-

ing, both as regards the numbers in attendance and the quality of the papers presented. The president of the society, Professor J. J. Kinyoun, occupied the chair at all of the sessions. The reports of the secretary and treasurer showed that the affairs of the society were in a healthful condition.

The nominating committee placed before the society the names of the following for election to the offices for the ensuing year, and they were unanimously declared elected:

President—Professor V. A. Moore, Cornell University.

Vice-president—Professor F. P. Gorham, Brown University.

Secretary and Treasurer—Professor C. E. Marshall, Michigan Agricultural College.

Councillors—Messrs. Prescott, Amyot, Stevens and Harris.

Delegate to the Council of the American Association for the Advancement of Science—Professor Erwin F. Smith.

The report of the committee on the identification of bacterial species was presented and adopted. As it is of an important nature, it is given here in full: (1) The standard card for the description of species has proved highly satisfactory in the hands of those who have given it a thorough trial. The maximum benefit from the card can only be realized, however, as its use becomes still more general. The committee, therefore, wishes again to urge upon the members of the society the great value of this method of recording bacterial characters. Advantage will accrue not only to the individual investigator, but in still higher degree to other workers on account of the comparability of the data thus obtained. (2) The numerical system of recording bacterial characters, while valuable for cataloguing cultures, must necessarily fail to approximate the natural classification of species. The method which at present seems most promising for determining the true relationships of these microorganisms is the statistical or biometric method. Bacterial species may most satisfactorily be defined by the quantitative study of measurable characters in a considerable series of cultures, the modal points or centers of frequency being given specific names, and larger groups having a number of common characters receiving the rank of genera or families. The committee urges upon the members of the society the importance of further systematic investigation along this general line. (3) In view of the great value of biometric classification to all workers in bacteriology the committee suggests the following resolution:

Resolved, That the Society of American Bacteriologists, recognizing the importance of a systematic study of bacterial species by the statistical method and the necessity for financial assistance in carrying out work which involves so large a proportion of routine, authorizes the committee on identification of bacterial species to present this need to any persons or institutions having charge of the distribution of funds for assisting scientific research.

(4) Finally, the committee believes that it would be of great advantage if descriptions and cultures of all new species or varieties could be submitted to some central bureau where they might be studied and compared and kept in such condition that bacteriologists could at any time obtain duplicate descriptions and subcultures for their own use. The committee therefore recommends the following resolution:

Resolved, That the Society of American Bacteriologists believes that the establishment of a central bureau for the preservation and distribution of descriptions and type specimens of the bacteria would be of great value to all workers in science.

F. P. Gorham
C.-E. A. Winslow

The council declared four vacancies to exist in the membership of the society and the following were elected to fill those vacancies: Dr. G. W. Stiles, Department of Agriculture, Washington, D. C.; Professor W. E. King, Kansas State College of Agriculture, Manhattan, Kans.; Professor R. E. Buchanan, Iowa State College of Agriculture, Ames, Iowa; Professor Oscar Klotz, department of pathology, University of Pittsburgh, Pittsburgh, Pa. The council also recommended that inasmuch as by the election of the foregoing persons the active list of membership was filled up, the wording of the constitution be altered to admit of an active membership of 150 instead of the present number of 125. This recommendation was favorably received and will be acted upon by the society at the next annual meeting.

On the evening of the twenty-ninth the society was the guest of the Boston Bacteriological Club at a "smoker" held in the rooms of the Technology Union, where the members thoroughly enjoyed themselves in an informal manner.

The program, as shown by the following titles and abstracts, was carried out and evoked much

interest and discussion, making the meeting one of the most successful that the society has yet experienced:

Some Observations on the Immune Body: J. J. KINYOUN, Health Department, Washington, D. C. (President's address.)

The president in the address delivered before the Society of American Bacteriologists gave a résumé of his observations on the several immune bodies in connection with the production of anti-sera and other substances associated with the phenomena of immunity. He claims that a distinction should be drawn between the specific anti-bodies as for example, the anti-toxin of diphtheria or of tetanus and other bodies which are also present in such sera. The claim is advanced that there is present in all such sera containing specific anti-bodies, others which may be termed common immune bodies. These have the property of increasing the resistance of the cells against many substances which are harmful to them and are of diverse origin. These common immune bodies are intimately associated with the leucocyte and it is believed that the leucocyte gives them origin. The statement is also made that the curative value of all anti-bacterial sera is due not so much to the specific anti-bodies, but to the common immune bodies which are always present.

An Improved Method of Employing "Antiformin" and Ligroin, in the Examination of Sputum, etc., for the Tubercle Bacilli: J. J. KINYOUN, Health Department, Washington, D. C.

The improvement of the method is in the simplification of the process both as to time and in manipulation. In the case of sputum, a small quantity of the "antiformin" forms one to three cubic centimeters, and about one cubic centimeter of ligroin (spec. grav. 0.715 to 0.720) is added at the same time. The sputum is placed in a shaker and shaken for about fifteen minutes, at which time the cellular contents and mucus are dissolved together with a greater proportion of the bacteria. A small quantity of this is placed in a centrifuge tube and spun at moderate speed for a minute or so to bring the ligroin to the top. The layer of saponified material lying at the juncture of the sputum and ligroin will contain nearly, if not all, the tubercle bacilli. Tissues can also be examined in the same way; small bits of the suspected tissue are placed in the antiformin together with ligroin and shaken in the same manner as for sputum, then centrifugalized and examined. This method is also well adapted for the examination of feces. The main advantage is the saving of time.

On the Production of Agglutinating Sera for Diagnostic Purposes: J. J. KINYOUN, Health Department, Washington, D. C.

The writer states that there is always an element of uncertainty in the production of anti-sera when the smaller laboratory animals are employed. And particularly is this true with the rabbit and guinea pig. It occurred to him to use medium-sized pigs (or shoats) for this purpose. These animals were found to be well adapted for the purpose as they withstood large quantities of living cultures of *B. typhoid*, *B. paratyphoid*, "*a*" and "*b*" and *B. coli* without the least discomfort. After two or three injections of any of these organisms above referred to, an agglutinating serum was obtained which reacted in high dilutions. The bleeding was from the tail; the quantity taken varied as to the size not less than 100 c.c. nor more than 400 c.c. at a bleeding. The animals are easy to handle, their maintenance is as cheap as smaller animals, and, moreover, they can always be depended upon not to die just before you complete the immunization.

Some Observations on the Fermentation of Silage: W. M. ESTEN, Storrs Agricultural Experiment Station.

The prevailing opinion of investigators in silage fermentation is that respiration and enzymes are the real agents in the process. These conclusions are maintained on account of finding high temperatures in fermenting silage and the forming of ensilage in the presence of chloroform and so forth. High temperatures are found in silage only when the surface is exposed to the air and where an alkaline fermentation is in progress, or when the per cent. of moisture is relatively low. Inside the silo, where acids are produced to preserve the silage, no high temperatures are found. When silage is formed by sterilizing with chloroform and so forth no acid is produced.

In cutting corn for ensilage each piece is covered with a film of sweet juice. In the subsequent filling of the silo the several tons of pressure forces out more juice, so that every piece and fragment of the silage is saturated and covered with a sugar fermentable substance, mostly dextrose. As is well known, all sweet fruit and plant juices undergo two types of fermentation, acid or alcoholic, and in some cases both occur together. The most common change of sugar to acid is by

lactic-acid bacteria. Some fruit juices, like apple cider, contain so much acid that nearly all kinds of bacteria are unable to grow in them. Apple juice contains about .72 per cent. of total acid. Corn juice has about .25 per cent. of unknown acid and some gallic acid (?). In corn juice lactic acid bacteria grow profusely till about .35 to .45 per cent. of lactic acid is formed, when they cease to grow. But yeasts are tolerant to much larger amounts of acid and therefore continue to grow in the corn juice till practically all the sugar is used up. The alcohol formed is mostly changed into acetic acid. In fresh silage large numbers of yeasts and acid bacteria are found. During the first twelve days of fermentation nearly all of the biochemical changes are completed. The maximum growth of acid bacteria is on the fourth day and the maximum growth of yeasts is on the twelfth day. The highest temperature of 29° C. was noted in the first 36 hours. The samples were taken from a hole in the silo five feet from the bottom, and from one to two feet from the edge.

Further Studies in the Acidity of Fresh Milk: W. M. Esten, Storrs Agricultural Experiment Station.

The entire range of variation of the acidity of the milk in a year for a herd of cows numbering more than twenty-five was from .155 to .187 per cent. The law of variation is that the acid of the milk varies inversely as the temperature. Approximately on the first of February the milk of all the cows is at its highest point of acidity. On the first of August it is at its lowest point of acidity. These two dates include the coldest and warmest periods of the year.

The variation during a lactation period proves to be quite remarkable. An acidity of .48 per cent. has been found at the first milking. In from two to three days it falls to about .25 per cent. from this figure, then gradually in about three weeks to the normal, near .17 per cent., which continues until about three weeks before the end of the lactation period, when, at the last milking, it falls to .12 or .13 per cent. The high acidity at the beginning is explained by the fact that the ash and salts are very much in excess of the normal amount. Some of these are probably calcium salts which are necessary for bone production in the young animal.

The quality of the milk varies as the acidity, so that winter milk has more food value than in summer and a higher price in winter is justified by this fact.

The acidity has an important bearing in the inspection of milk. A dairy selling Jersey milk with 5 per cent. of butter fat will sometimes show an acidity of .20 per cent when fresh, and does not then contain a particle of lactic acid. Under these conditions the milk should not be condemned for high acidity, but rather recommended for the high acidity which indicates a high quality. It is therefore requisite that milk inspectors be capable of judging the high acidity of milk which indicates high quality and value, from high acidity caused by growth of acid organisms which produce lactic acid in milk of any quality.

Bacteriological Methods in the Oyster Survey of Virginia: Meade Ferguson, Laboratories of State Board of Health of Virginia. (Read by title.)

Methods of Testing Shellfish for Pollution: Stephen DeM. Gage, Massachusetts State Board of Health, Experiment Station, Lawrence, Mass.

The methods for testing shellfish for pollution in Massachusetts have been devised to facilitate the routine handling of a large number of samples in the easiest and most accurate way.

Collection of Samples.—Twelve to fifteen shellfish are collected from each sampling station, in wide-mouth spring-top glass jars. The sampling stations are distributed well over the area from which shellfish are gathered, and samples of sea water are collected from each station in addition to the shellfish samples.

Transportation of Samples.—Shellfish samples should be delivered at the laboratory within twenty-four hours after collection. Packing samples in ice is probably unnecessary, except during very hot weather.

Technic of Testing.—The individual shellfish is washed with sterile water, opened with a sterile oyster knife and a portion of the shell water transferred to a fermentation tube. The body of the shellfish is then removed from the shell, washed with sterile water, opened with a sterile scalpel and a portion of the alimentary canal transferred to another fermentation tube. Ten individual shellfish from each sampling station are tested in this manner.

B. coli Methods.—Dextrose pepton water is used in the fermentation tubes. The incubation of these tubes and the isolation and confirmation of *B. coli* are in accordance with the standard methods used in water analysis. No systematic search is made for the sewage streptococcus, but

if its presence is noted either on the plates or on the agar streaks, this is recorded.

Interpretation.—Tests of a single shellfish from any location have little diagnostic value. When a sufficient number of shellfish have been tested, the absence of *B. coli*, or of positive fermentations followed by an overgrowth of sewage streptococcus in 80 per cent. of the samples tested, indicates that the location is reasonably free from pollution. If 50 per cent. or more of shellfish from a location show *B. coli*, or fermentation overgrown by sewage streptococcus, the location is dangerously polluted. Between these limits, the interpretation is a question of degree of pollution, based on individual judgment, into which analyses of the sea water from the same source, and a sanitary inspection of the source must enter.

Some Peculiarities in the Counts of Bacteria at 20° C. and at 40° C. from Waters Treated with Disinfectants: STEPHEN DEM. GAGE, Massachusetts State Board of Health, Experiment Station, Lawrence, Mass.

For some years we have been making counts of the bacteria at 40° C. in addition to the usual count at 20° C., and have found that with natural waters and the effluents from good water filters there is an approximately constant ratio between the counts at the two temperatures. For example, effluents from good water filters, and surface and ground waters used as public water supplies in Massachusetts, usually contain less than 100 bacteria per cubic centimeter according to the 20° C., less than 10 per cubic centimeter, as shown by the 40° count, and about half of the latter will produce red colonies on litmus lactose agar.

When dealing with waters, etc., which have been treated with certain disinfectants such as bleaching powder, whose efficiency is produced by oxidation, we have frequently found that while the numbers of bacteria determined at 20° C. might be reduced to less than 100 per cubic centimeter by a small amount of disinfectant, frequently there would be no corresponding decrease in the 40° count, and that considerably more disinfectant must be used to make the 40° count conform to the standard of the good waters, as previously stated.

Furthermore, in a great many instances the 40° count on disinfected waters was as high or higher than the 20° count. This phenomenon has occasionally been observed with natural waters and sewages, but an analysis of the records of many thousand samples shows that the percentage of such samples is not over five per cent. On the other hand, 20 to 25 per cent. of samples of water and 50 to 70 per cent. of samples of sewage and effluents from contact and trickling filters, after treatment with bleaching powder, showed higher counts at 40° than at 20° C. This abnormally high 40° count is seldom found when the 20° count is high, but when the latter count is below 100, these peculiar results are frequent.

These abnormal ratios with disinfected waters are not peculiar to Massachusetts, but have also been noticed elsewhere where bleach disinfection has been tried. In many instances, however, such results appeared to be so erratic that they were considered to be abnormal and were thrown out, and we so considered them at first. When we found that they occurred with a frequency of 20 to 70 per cent., however, we did not feel justified in calling them abnormal or in throwing them out.

It can be stated definitely that this phenomenon of abnormal ratios is not due to spores. A careful study of this point has been made, and the ratio between total colonies and spore formers at both 20° and 40° has been proved to be practically the same before and after disinfection.

Diphtheria Bacillus Carriers in the Public Schools: F. H. SLACK, B. L. ARMS, E. M. WADE and W. S. BLANCHARD, of the Bacteriological Laboratory of the Boston Board of Health.

This paper presents the details and results of an experiment undertaken at the beginning of the school year in the Brighton District of Boston, Mass.

The pupils in this district number over 4,000 and two cultures were taken from each during two successive weeks. All microscopic examinations were made in the bacteriological laboratory of the Boston Board of Health by the regular corps of workers.

Positive results were reported only on those cultures showing the A, C or D types of organisms (Wesbrook).

On the first day, 1,287 cultures were examined; the second, 1,131; the third, 1,029; the fourth, 699—a total of 4,146, and of these 55 or 1.33 per cent. were positive.

These cases were for the most part removed from school.

The second round the following week gave 1,275 cultures the first day; 1,113 the second; 1,029 the third, and 670 the fourth—a total of 4,081, of which 38, or .93 per cent., were positive.

Details concerning these cases and a five-year chart of clinical cases in the district are given.

The following conclusions are reached:

1. That at least 1 per cent. of all healthy school children are carriers of morphologically typical diphtheria bacilli (Wesbrook's A, C, D types).

2. That such bacilli are communicable from one to another and the condition is usually a transient one.

3. That the organisms are ordinarily of little or no virulence.

4. That while it is possible, by passing through a susceptible individual, their virulence might be raised to cause the disease, this is not a frequent occurrence.

5. That the disease diphtheria is kept alive in a community rather by virulent organisms in immune persons than by these non-virulent bacilli.

6. That where *virulent* diphtheria bacilli are present as shown by outbreaks of the disease, cultural tests of all contacts and isolation of those showing positive cultures is a duty owed to the community.

7. Where the *disease* does not exist, isolation of carriers of probable *non-virulent* bacilli is of no proven benefit, and is a costly and laborious procedure entailing much unnecessary hardship on innocent and probably harmless parties.

8. The attempt to control diphtheria in a city by a round of cultures from all school children at the beginning of the school year does not seem encouraging from this series of tests.

9. The proposition to stamp diphtheria out of a city by cultural tests of all the inhabitants and isolation of all carriers is impossible from any practical standpoint.

The Virulence of Old Cultures and Subcultures of B. mallei: B. L. Arms, M.D., Assistant Director of the Bacteriological Laboratory of the Boston Board of Health.

From work done at the Boston board of health laboratory the following conclusions are drawn:

1. That in glycerine broth, *B. mallei* live and retain their virulence for at least two months, even when kept at body temperature.

2. That a culture of *B. mallei* may be virulent after growing on potato for at least a month.

3. That some stains of *B. mallei* retain their virulence through a great many subcultures on artificial media.

How shall the Value of Disinfectants be Determined? E. M. Houghton, Detroit, Mich.

Some Observations on the Wassermann Reaction: Lawrence T. Clark, Detroit, Mich.

Departures from the principles upon which the original Wassermann method for the serum diagnosis of syphilis is based are fraught with the dangers attending unreliable and, in many cases, entirely erroneous results. In making the test a thorough knowledge of the strength and keeping qualities of the various factors entering into it is very essential. Such facts, enumerated in the conclusions, make it possible to diagnose a high percentage of doubtful cases with a considerable degree of accuracy.

Conclusions.—The complement content of fresh guinea-pig serum varies materially with different pigs.

More uniform and accurate results are obtained when the guinea-pig serum is standardized to known normal and syphilitic sera before doubtful samples are tested.

Hemolytic serum kept at uniform low temperature retains its activity for a relatively long time, although it loses some of its original strength and needs restandardizing from time to time.

Suspensions of thoroughly washed red blood corpuscles (ram) kept at 1.6° C. have been used up to fourteen days after drawing with good results.

Samples of serum inactivated (56° C. one half hour) and kept free from contamination, remain unchanged for several days. This enables one to store samples and run several at one operation.

Practical tests made with properly standardized reagents gave 95 per cent. and 93⅓ per cent. accurate results in known and doubtful cases, respectively.

Negative reactions were obtained after vigorous specific treatment in eight cases which gave positive reactions before treatment. A future publication will deal with this phase more extensively.

It would seem to be indicated by results from the limited number of cases tested that the complement fixation reaction, when carefully carried out and thoroughly controlled, is a reliable means for diagnosing the doubtful case.

The Usefulness of Curves in the Interpretation of Biochemical Processes: Otto Rahn, Michigan Agricultural College.

If a curve of a biochemical process is plotted, taking as abscissa the time elapsed and as ordinate the total amounts of compounds produced, the shape of this curve will in many instances indicate the nature of the change taking place. In a purely chemical or enzymatic change, the active mass does not increase, and therefore the rapidity of the process measured by the angle of elevation of the curve does not increase. (Under enzyme is understood a chemical compound, unable to multiply.) The curve changes with the

time, becoming more and more parallel to the base line. If we are dealing with changes caused by microorganisms, the active mass is increasing as long as microorganisms increase, and consequently the velocity of the process, or the angle of elevation, will rise as long as the increase continues. This elevation of the curve is characteristic for compounds produced by any multiplying organism. From the time the increase ceases, we are dealing with a purely enzymatic curve.

The exact plotting of the curve allows us to make fairly accurate statements about the multiplication and the duration of the increase of bacteria, even if they can not be counted by our present methods. The point of inflection of a curve shows the moment when the organisms producing the substance under study reach their maximum number and can be studied with the greatest convenience.

In some instances, the point of inflection is changed to a straight line, indicating a very resistant strain of bacteria; this seems to take place especially in poor media, as soil extracts. A few experiments indicate that poorly nourished bacteria are able to produce a larger amount of fermentation products than well-nourished bacteria, though they need a much longer time to accomplish it.

The Society Card as a Basis for Classifying the Bacteria producing Soft Rot in Vegetables: H. A. HARDING and W. J. MORSE, New York Agricultural Experiment Station.

This group includes *B. carotovorus* Jones, *B. aroideæ* Townsend, *B. omnivorus* van Hall, *B. oleraceæ* Harrison and some other described forms.

A comparison on the basis of the society card brings out the fact that these described cultures are identical in all cultural characters except the results from the fermentation tube.

Extended study of this point indicates that this difference is more apparent than real, since the normal gas-forming ability of this group lies so near the amount required to saturate the fermentation tube that the appearance of visible gas varies with the fermentative vigor of the particular culture.

These results indicate that in cases where the fermentative ability of a culture is weak there is need of a more accurate instrument than the fermentation tube for accurately detecting gas formation.

(Data to appear as New York Agricultural Experiment Station Technical Bulletin 11, 1909.)

Does the Group Number on the Society Card Carry the Classification far enough to Break up the Species? H. A. HARDING, New York Agricultural Experiment Station.

This point was tested with approximately fifty strains of *P. campestris* (Pam.) Smith. This species was chosen because it is a well-known, chromogenic, plant-pathogen in which the limits of the species can be determined with the minimum chance of error.

Some of the tested strains were freshly isolated from the host while others had been cultivated in various laboratories for many months. The larger part of these cultures were revivified just previous to being tested and were tested on standard media. In some cases these precautions were purposely omitted. Independent observations were made in some cases by three different workers and media prepared by three different persons was used.

With the exception of the reduction of nitrate there was no variation in the group number as determined from these cultures.

The variation in nitrate reduction, as determined by the official method for nitrite, was apparent rather than real since it was not shown by the nitrite test with the starch, KI, H_2SO_4 test. The faint reactions obtained with the official test were undoubtedly due to absorbed nitrite. Nitrite is not absorbed equally by all tubes and a large number of check tubes must be held to insure accurate comparisons in faint reactions.

A New and Improved Method of Enumerating Air Bacteria: LEO F. RETTGER, Yale University.

Studies on Bacterial Mutation: LEO F. RETTGER, Yale University.

A Comparative Study of Intestinal Streptococci from the Horse, the Cow and Man: C.-E. A. WINSLOW and G. T. PALMER, Massachusetts Institute of Technology.

Andrewes and Horder's statistical study of the streptococci has for the first time made it possible to classify the principal types of this complex group in a fairly satisfactory manner. One of the most interesting points about Andrewes and Horder's classification, and the earlier observations of Gordon and Houston on which it was founded, was the apparent difference between streptococci from the intestines of the horse, the cow and man. In the present investigation we have tested this point by isolating one hundred strains of streptococci from fæces of each of the three animals; we have cultivated them in broth containing four different fermentable media (dextrose, lactose,

raffinose and mannite), and determined by titration the amount of acidity produced by each strain in each medium. An examination of the results obtained confirms and harmonizes the work of the English observers in all particulars. The commonest streptococci in human fæces are *S. mitis* (acidifying dextrose and lactose), *S. fæcalis* (dextrose, lactose and mannit) and *S. equinus* (dextrose alone). In the fæces of the cow *S. equinus* and *S. mitis* are present; but *S. fæcalis* is absent and a form rare in human fæces, *S. salivarius* (dextrose, lactose and raffinose), is fairly abundant. In the fæces of the horse practically all the streptococci present are of the *S. equinus* type. (Full paper, *Journal of Infectious Diseases*, VII., 1.)

The Determination of the Number of Leucocytes in Milk by a Direct Method: S. C. Prescott and R. S. Breed, Boston, Mass.

The methods in general use for determining the number of leucocytes present in milk are all based on the use of the centrifuge. The assumption is that all but a small fraction of the leucocytes are precipitated and also that this fraction is a fairly constant proportion of the whole and can safely be neglected. An investigation carried on in the Boston Biochemical Laboratory during the past summer has shown both of these assumptions to be incorrect. By the use of a new method, it has been found that the distribution of the leucocytes in a given sample of milk after centrifuging varies greatly in different samples of milk, although their distribution is approximately the same in different samples of the same milk. Usually more than half are present in the cream, one fourth or less in the precipitated slime, and the remainder in the skim milk.

The variation in position of leucocytes in different samples is apparently due to the variable percentages of cream present. The distribution of the leucocytes in a centrifuged sample corresponds closely to the previously known distribution of bacteria in similar samples.

The new method by which these facts have been ascertained is as follows: a measured drop (.01 c.c.) of milk to be examined is spread evenly over a measured area (1 sq. cm.) on a glass slide, dried with gentle heat, the fat dissolved out with xylol, fixed with alcohol for a few minutes, the slide again dried and over-stained with methylene blue and partially decolored with alcohol. The number of leucocytes present is then determined by examination with the microscope. Results done in duplicate show a small percentage variation proving that the practical error is not a large one.

A series of tests of milk show that much larger numbers of leucocytes are normally present in milk than has been supposed. The average number of leucocytes present in the samples examined is approximately 1,500,000 per cubic centimeter, while numbers less than 100,000 per c.c. are uncommon.

The Bacteriology of Condensed and Evaporated Milks: S. C. Prescott and R. N. Hoyt, Massachusetts Institute of Technology.

Some Problems of Sanitary Milk Production: P. G. Heinemann, A. B. Luckhardt and A. C. Hicks, The University of Chicago.

A series of experiments was made during the month of September at a sanitary dairy to throw light on the following points: (1) the bacterial content of separator milk and cream, (2) the value of narrow top pails with and without strainers, (3) the bacterial content of milk after straining through layers of absorbent cotton, (4) a study of body cells in separator slime and an attempt at classification.

It was found that the bacterial content of the separator cream was very small, the average of 48 tests being 132 bacteria per cubic centimeter of 40 per cent. separator cream. The separator milk in the same number of tests contained 2,130 bacteria per cubic centimeter and the original milk contained 738 bacteria per cubic centimeter. We conclude from these experiments that the action of the separator tends to break up clumps, chains and imperfectly divided forms so as to increase the colony count.

The experiments with the narrow top pail with and without strainer showed that the count was 620 bacteria per cubic centimeter with the strainer and 674 without the strainer. These figures are the averages of 108 tests. The small difference in favor of the strainer may possibly be due to experimental error and of little significance. Still we think that the strainer should not be omitted, since the milk needs straining at some point or other of production to remove foreign material which is bound to gain access even in the most carefully managed dairies.

As a result of 240 consecutive tests we conclude that straining milk through thick layers of absorbent cotton, as is customary in many dairies, is decidedly disadvantageous. The force of the milk being poured on top of the strainer seems to break up bacterial aggregates so as to increase the colony count in the strained milk.

Our study of the body cells in the separator slime has led to the following conclusions:

1. Polymorphonuclear leucocytes of the neurophile type, large mononuclear leucocytes, and small lymphocytes appear normally in the separator slime of the milk of healthy cows, and as far as we can see they bear no relation to the number of microorganisms present, including streptococci.

2. Eosinophiles may occur in the slime of the separator. The cause and significance of their presence remains problematical.

3. The white corpuscles in milk of normal and diseased cows, and in the blood of the same animals, ought to be studied, differentiated and classified. Such a study will put the subject of leucocytes in milk on a more exact scientific basis than heretofore, and further our knowledge on the significance of the relative number of the various corpuscles in milk in normal and diseased conditions of the cow in general, and in pathological processes of the mammary glands and the udder in particular.

The details of our experiments and a critical discussion of previous work will appear in the January number of the *Journal of Infectious Diseases*.

A Bacterial Disease of Alfalfa caused by Pseudomonas medicaginis (Sackett) n. sp.: WALTER G. SACKETT, Agricultural Experiment Station of Colorado.

The disease has been known in Colorado since 1904, where, in some localities, it has caused the loss of practically 80 per cent. of the first cutting.

In the earliest stages, the stems have a yellowish, olive-green color and appear watery and semi-transparent; soon the color changes to an amber, due to the appearance and subsequent drying of a thick, clear exudate. This dried excretion gives the stem a shiny, varnished appearance, and a slightly rough feel to the touch. These stems blacken in six to eight weeks, become very brittle and are easily broken, which fact makes it almost impossible to handle the crop without an immense amount of shattering.

So far as our observations go, the disease is confined principally to the stem and lower leaves; it appears to run its course with the first cutting, and those plants which have sufficient vitality throw out a good growth for the second and third cuttings.

The disease has been shown to be due to a bacterium which lives in the soil, presumably, and this infected soil enters the plants through cracks in the epidermis which are caused by freezing.

Brief characterization: The causal organism is a short rod with rounded ends, size $1.2 \times .7\ \mu$, actively motile by 1–4 bipolar flagella, non-spore-forming and to which the writer has given the name *Pseudomonas medicaginis*, n. sp. The organism forms filament but no capsules; Gram negative. Surface pellicle in broth; shining grayish white on agar, fluorescent green after three days; gelatin colonies round, gelatin stab-surface growth only, no liquefaction; potato discolored, moderate growth, orange yellow, starch not destroyed; no growth 37.5° C.; no growth in Cohn's solution; growth in Uschinsky's solution. No liquefaction; rennet curd 40 days, no peptonization 25 days; no indol or hydrogen sulphide; nitrates not reduced; ammonia from peptone and asparagin; fluorescent. Habitat—soil. Pathogenic for alfalfa. Classification, Ps. 212.3332133.

This paper, in full, is now in press as a bulletin of the Colorado Agricultural Experiment Station.

A Comparative Test of Several Synthetic Media for the Isolation of B. coli: H. W. LYALL, Brown University.

The three media studied were Harrison and Vanderleck's esculin medium,[1] Dolt's asparagin medium[2] and Dolt's malic acid medium.[3]

The total count, the number of red or black colonies, and the per cent. of these which proved to be *B. coli* were determined.

The results are given in the following table:

Medium.	Source of Sample	Total Count	Average Number of Red or Black Colonies	Per cent. of Colon
Standard. Lactose agar.	A	527	20	50
	B	84	2	
Esculin.	A	105	26	64
	B	30	2	
Asparagin.	A	90	7	88
	B	24	1	
Malic acid.	A	23	5	94
	B	7	2	

A = Pettaconsett intake—Pawtuxet River.

B = East Providence intake—Ten Mile River.

The conclusions drawn were that the esculin medium is of about the same value as the standard litmus lactose agar over which it has no advantages, the asparagin agar gives a much

[1] *C. f. B.*, I., Orig. 51, 1909, 607.

[2] *Jour. Inf. Dis.*, 5, 1908, 616.

[3] *Ibid.*

higher percentage of colon colonies than either the standard litmus lactose agar or the esculin medium, and is a very favorable medium for colon isolation, the malic acid agar gives a still higher percentage of colon colonies, combined with a very low total count, and is a very satisfactory medium for colon isolation when only the active forms indicative of recent pollution are desired.

Studies in Soil Bacteriology, IV.: The Inhibition of Nitrification by Organic Matter, Compared in Soils and in Solutions: F. L. Stevens and W. A. Withers, assisted by P. L. Gainey, J. K. Plummer and F. W. Sherwood, North Carolina College of Agriculture and Mechanic Arts.

In experiments regarding nitrification it was demonstrated that:

In Liquid Medium

Peptone 0.8 per cent. inhibited at 4 weeks.

Cottonseed meal, nitrogen equivalent, 0.1 per cent. inhibited at 4 weeks.

Cottonseed meal, nitrogen equivalent, 0.1 per cent. inhibited at 16 weeks.

Peptone 0.8 per cent. inhibited at 16 weeks.

In Liquid Medium absorbed by Soil

Peptone 1.25 per cent. retarded at 4 weeks.

Peptone 5 per cent. inhibited at 4 weeks.

Peptone 5 per cent. did not retard at 16 weeks.

Cottonseed meal, nitrogen equivalent, 0.4 per cent. retarded at 4 weeks.

Cottonseed meal, nitrogen equivalent, 0.5 per cent. retarded at 16 weeks.

Cottonseed meal, nitrogen equivalent, 0.1 per cent. did not retard at 4 weeks.

Cow manure in quantities equivalent to 1, 5, 10, 20, 30, 40, 80, 160 tons per acre in 2-week, 8-week and 12-week periods, did not retard nitrification in soil at 8 weeks but rather favored it.

Nitrification occurred in pure cow manure at 8 weeks and at 12 weeks.

A Simple Low-temperature Incubator: Karl F. Kellerman, Bureau of Plant Industry, Washington, D. C.

During the winter of 1905 the writer found it necessary to improvise a low-temperature incubator, and since that time has had similar ones in almost constant operation. The incubator is fundamentally a four-compartment refrigerator carrying ice in the upper right-hand compartment and a heater, consisting of a single incandescent electric-light globe and a thermo-regulator, in the lower left-hand compartment. The thermo-regulator may be operated on a separate circuit using a storage battery or Edison-Leland cell, or from a shunt circuit from the main feed wire. Such incubators may be installed readily and may be used for either temporary or permanent purposes.

Flagella Staining of Pseudomonas radicicola (B.) Moore: Karl F. Kellerman, Bureau of Plant Industry, Washington, D. C.

Staining unfixed smears of *Pseudomonas radicicola* with saturated alcoholic stains according to the method of Edwards and Barlow has occasionally given the figures described by these authors as indicating the polar flagella or "giant whips." I have been able to duplicate these appearances almost exactly by mixing bacteria which had no polar flagella with artificial slime or gum and preparing and staining the slides according to the method of Edwards and Barlow. Therefore, while this method of staining may have a diagnostic value for the peculiar slime secreted by *Pseudomonas radicicola*, I do not believe that the flagella themselves are indicated.

Nitrification Studies in Nevada and Utah: Karl F. Kellerman, Bureau of Plant Industry, Washington, D. C.

In the arid and semi-arid regions of the west there are two areas which in many ways should be comparable; these areas are the beds of the prehistoric Lake Bonneville in Utah and Lake La Hontan in Nevada. The former, containing the Mormon settlements, has long been famous for the crop-producing power of its soil; the latter has furnished some valuable farms to early ranchers, and recently considerable areas have been brought under irrigation as the Truckee-Carson project.

During the past year nitrification studies have been carried on in Utah by Mr. I. G. McBeth and in Nevada by Mr. E. R. Allen. These studies have indicated that nitrification proceeds in Utah soils following the same general rates of activity in the different layers that occur in eastern soils, nitrate formation decreasing very rapidly below the surface, although it persists to much greater depth than has usually been described. Azotobacter is very frequent and occurs in appreciable numbers even as far below the surface as the tenth foot. In the Nevada soils nitrification is very erratic, the surface layers in many cases nitrifying much less rapidly than deeper layers, while some regions seem to lack the nitrifying flora almost completely. The deficiency in nitrifying bacteria seems correlated with poor crop production in many cases, although the prevalence of alkali is also a source of crop injury. When not present in excessive quantities the white

alkali does not seem to be the controlling element in the nitrification processes, and judging by the innocuous action of gypsum it seems evident that in these regions black alkali is not sufficiently abundant to be an important factor. In the deeper layers of the most unproductive soils a peculiar fungus is very prevalent and is probably associated with the failure of this soil to produce crops.

Desiccated Culture Media: W. D. FROST, University of Wisconsin.

In order to overcome the generally recognized faults of bacterial culture media, such as variation in the composition of small batches, time consumed in preparation, rapidity with which it deteriorates, and its unavailability in small institutions or private practise, the preparation of culture media in large batches in establishments especially equipped for it and then desiccated, is suggested.

The author's work on this problem, covering nearly a decade of time, is considered and samples are submitted.

There is, apparently, no reason why the different culture media can not be put upon the market in a form which requires merely the addition of water and sterilization to make it ready for use. Not only the ordinary, but probably most of the special media, can be prepared in this way and could be put up, where desired, in the form of tablets, these to be of such a size that they could be put directly into test tubes, and when the proper amount of water is added they would be ready for sterilization and use.

Laboratory Desks for Students in Bacteriology: W. D. FROST, University of Wisconsin.

A laboratory desk is described for use in student laboratories for which it is claimed that the maximum number of students can be accommodated in given quarters with, probably, the minimum of confusion. The desk is similar to those used in chemistry, without the shelf above it. It is provided with a wide trough running the full length, which serves as a sink and over which gas hot plates are placed to be used in cooking and sterilization. Each place is provided with three lockers which can be used by as many different students. The reagent shelves are provided for and a shelf for rough weighing at one end, and at the other end the hot-air sterilizer and autoclave. Microscopical work can be done at each place by using artificial light, or at small window desks, provided for separately.

An Inexpensive Incubator Room: W. D. FROST, University of Wisconsin.

A small room is used and maintained at a satisfactorily constant temperature without other change than the attachment to the steam radiator of a thermo-regulator designed for residences. The cost was about thirty dollars, and, with proper shelving, the room will accommodate several hundred students. An arrangement of lockers is also suggested which largely removes the temptation of students to appropriate cultures not belonging to them.

The Absolute Relation of B. coli to Oxygen: F. G. KEYES, Brown University.

The absolute relation of gaseous oxygen to the growth and gas production of *B. coli* promises to be somewhat complicated.

In a study of the absolute gas production of *B. coli* in vacuo,[4] it was found that the gas evolved from a 1 per cent. asparagin, 0.2 per cent. disodium phosphate, 1 per cent. dextrose medium began to fall off very decidedly after 115 hours, but the gas evolved was constant in composition.

In the presence of pure oxygen, no other gas being present, it is found that the rate of evolution of gas is much smaller, but gas production continues for a much greater length of time. The composition of the evolved gas is different when the organism is grown in the presence of oxygen from what it is in vacuo. The composition of the gas depends to a certain extent upon whether the medium is neutralized or not. Some oxygen is absorbed by the growth of the organism in the medium.

The tables below summarize the results of the experiments.

The Absolute Gas Production of B. Typhosus: L. J. GILLESPIE, Brown University.

While attempts were being made to find a synthetic medium suitable for the growth of *B. typhosus* and such as to facilitate a comparison with the gas production of *B. coli*, preliminary experiments were made using a medium containing Witte's peptone. *B. typhosus* was grown on a neutral medium containing 1 per cent. dextrose and 1 per cent. peptone; and the gas evolved, obtained by the procedure given by Dr. F. G. Keyes, was analyzed. The amount of gas found was so small that analyses with the ordinary 100 c.cm. gas burette and gas pipettes were carried out with difficulty. The analyses indicated, however, the following results for 48 hours' growth:

[4] *Jour. Med. Res.*, 16, 1909, 69.

ABSOLUTE GAS PRODUCTION

In Vacuo (Medium not neutralized)					In Oxygen (Medium neutralized with NaOH)					
Hours in Incubator	Total Per Cent. of Gas	Composition of Gas			Hours in Incubator	Total Per Cent. of Gas	Composition of Gas			Oxygen Absorbed in c.cm.
		CO_2	H_2	N_2			CO_2	H_2	N_2	Dry 20° C. 760 mm.
24	26.7	63.23	36.61	0.15	25	15.17	98.83	0.18	0.99	1.57
48	45.6	63.27	36.05	0.37	127	18.56	97.90	0.52	1.58	2.55
115	99.9	63.49	35.81	0.70	167	27.18	98.21	1.06	0.73	5.52

Total gas per 100 c.cm. medium ..... 1.4 c.cm.
CO_2 ..... 92%
H_2 ..... 3%
N_2 (by difference) 5%

By means, however, of an improved gas analysis apparatus devised by Dr. Keyes it was found possible to analyze accurately such small amounts. The results for 15 days' growth were as follows:

	Exper. I.	Exper. II.
Total gas obtained ...	3.49 c.cm.	5.89 c.cm.
Total gas per 100 c.cm. medium	2.55 c.cm.	2.32 c.cm.
CO_2	93.25%	97.10%
H_2	2.03%	2.12%
N_2 (by measurement)	4.44%	.77%
	99.72%	99.99%

Experiment II. probably approaches nearest the truth, as the leakage of only a trace of air into the bulb during incubation would easily suffice to falsify the figure for nitrogen.

Substitutes for Löffler's Bloodserum for the Diagnosis of Diphtheria: W. W. Browne, Brown University.

Attempts were made to grow the diphtheria bacillus on Hadley's medium,[5] solidified with 5 per cent. agar. Growth on this medium was atypical. Albumen was then substituted for the glycocoll of Hadley's medium, but on sterilization the albumen was coagulated as a flocculent precipitate through the medium. The growth was scanty. Attention was now turned to egg as a source of albumen. Egg was mixed with dextrose broth in the ratio of 1 part of broth to 3 of egg and coagulated. The medium was hard and firm. The slow growth of the bacillus on this medium would prevent its use in the diagnosis of diphtheria.

Alkaline albuminate was then substituted for the albumen. Although the albumen was not coagulated by heat, nevertheless, it was precipitated by the acid produced by the bacillus. The growth on this medium was satisfactory, but the precipitation of the albumen prevented ready diagnosis. Acid albumen, on the other hand, showed scanty growth.

Next, a 6 per cent. solution of commercial albumen was mixed with dextrose broth in the ratio of 1 part of broth to 3 of albumen. This medium when coagulated presented a hard firm surface. The bacillus seemed to grow well upon it and diagnosis from throat cultures was fairly easy. While this medium does not seem to be a perfect substitute for the Löffler's serum, yet, in times of scarcity, it might be used to good advantage.

The Hygiene of the Swimming Pool: John W. M. Bunker, Brown University.

The swimming pool is liable to be a source of contagion if the water is used for any length of time. Cases of ear and nose affections have been traced to this source. Typhoid is apt to enter the pool and spread therefrom, and unless an abundant supply of water is available, the expense of frequent renewal is prohibitive.

Filtration is the method of purification ordinarily employed, but usually yields only partial purification, inasmuch as only a small part of the water of the pool is removed, filtered and returned to the pool. The filter at Brown University has a high efficiency and keeps the water of the pool a good color, but the bacterial content of the pool is always high.

Sterilization by heat is out of the question because of expense. Sterilization by the addition of chemicals has proved effective in the case of

[5] *Journal of Infectious Diseases*, Supp. 3, May, 1907, p. 95.

sewage and water, chlorine being especially effective.

The application of this method to swimming-pool water was tried, with the result that hypochlorite of lime in quantities sufficient to give one part available chlorine to two million of water gave efficient sterilization. The pool when so treated remained practically sterile for four days, during constant use. No odor or taste from the chemicals was noticeable. How often such treatment need be applied must vary with local conditions.

Probably for the ordinary swimming pool, if these experiments are borne out by experience, the addition of hypochlorite of lime, in the proportion of one part available chlorine to two million of water, twice a week, would insure a practically sterile pool.

A New Device for the Isolation of B. coli: W. F. WELLS, Massachusetts Institute of Technology.

A laboratory device was described which combines certain advantages of both dextrose broth and lactose bile. It consists of two Durham tubes (the first containing an enrichment medium, as dextrose, the second a selective medium, as bile) so connected by a capillary that the production of gas in No. 1 immediately causes a flow into the bile tube. As the capillary leads from the upper part of the inverted inner tube in No. 1, further increase in gas lowers the liquid below the mouth of the capillary and the flow cuts itself off.

If water containing *B. coli* is put into the dextrose tube the non-motile and aerobic bacteria remain outside the smaller inner tube, while *B. coli*, swimming continually in search of a better medium, finds its way around; so it is likely that such organisms will reach the portion about the mouth of the capillary very soon. With every advantage they multiply rapidly, and in a few hours the inverted tube contains a seething culture of vigorous *B. coli*, and gas forms quickly. The change in level causes a flow into the bile tube, just at the time of most vigorous growth, and then cuts itself off. The bile now contains an almost definite measure of thriving *B. coli*, probably in pure culture. Under these definite conditions the quantity of gas produced should be regular, and the per cent. formed in a given time after the first tube ferments significant.

The tubes are handled almost as simply as ordinary tubes. They are clamped together; a small test tube is hooked into the short leg of the capillary, while the longer legs straddle into both large tubes. They are made up and sterilized as usual, filling upon cooling.

The double medium secures the advantages of both. It does more; it preserves *B. coli* at its most favorable stage, the moment of gas production, and inoculates the bile under definite conditions with a dose of healthy organisms. It may be reasonably expected that the gas formers which are accustomed or can accustom themselves to the digestive tract will be indicated. Practical results show no unexpected error in the reasoning, and as far as they go promise an efficient test.

NORMAN MACLEOD HARRIS,
Secretary

UNIVERSITY OF CHICAGO

SOCIETIES AND ACADEMIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 677th meeting was held on February 26, 1910, Vice-president Abbot in the chair. The following paper was read:

The Recovery and Discussion of the Earliest Magnetic Observations along the Antarctic Continent and in the Approaches to the South Magnetic Pole: Mr. G. W. LITTLEHALES, of the U. S. Hydrographic Office, Navy Department.

The results were chiefly in a chart of terrestrial magnetic lines for the epoch 1840, representing the inclination and the declination of the magnetic needle founded upon the observations of the United States exploring expedition which discovered and traversed the coast of the Antarctic continent in about 66° of south latitude between the 160th and 97th degree of longitude east of Greenwich, in the beginning of the year 1840.

The observations presented have but lately been recovered from among a part of the records of the exploring expedition of which all trace was lost for many years, and they have resulted in the portrayal of a passing state to which we could not otherwise have reascended.

Such magnetic lines from original observations made long ago have a value which increases with the lapse of years on account of their importance in elucidating the changes which time works in altering the magnetic state of the earth.

The interpretation of the results proves that American explorers were the first to point out the region of the south magnetic pole by disclosing its presence, at that epoch, as an area of considerable extent, over which the dipping needle stood vertical or nearly vertical, around a position in 68° 50′ of south latitude and in longitude 135° east of Greenwich.

Dr. W. J. Humphreys, of the U. S. Weather Bureau, then spoke informally on "Solar Disturbances and Terrestrial Temperatures."

The speaker's purpose in this paper was to bring harmoniously together, as cause and effect, some solar and terrestrial phenomena.

The sun being the source of practically all of the radiant energy we receive, any change in its surface that affects its radiation must, through the resulting modification of the energy received, also affect certain terrestrial phenomena, some of which are of vital importance.

The speaker briefly discussed the relation of the changes in the number and extent of sun-spots, flocculi and coronal streamers to such terrestrial phenomena as auroral displays, magnetic storms, temperature changes and plant growth, and pointed out how some of these relations may be explained.

The following conclusions were reached in reference to the relation of changes in sun-spots and auroral discharges to terrestrial temperatures:

1. An increase in sun-spots appears certainly to be accompanied by a decrease in terrestrial temperatures fully twenty fold that which can be accounted for by the decrease in radiation from the spot areas alone.

2. It seems nearly certain that sun-spot maxima, whatever the value at such times of the solar constant, must lead to a decrease in the ultra-violet radiation that reaches the earth, and a corresponding decrease in the production, by this method, of ozone in the upper atmosphere.

3. The increase in the auroral discharges that accompany spot maxima tend to increase the amount of ozone.

4. The change in temperature of the earth, and all its train of consequences, from spot maximum to spot minimum, is not necessarily dependent upon a change in the solar constant. It may depend largely, if not wholly, upon a change in the absorptive property of the atmosphere, caused, we believe, by a variation in the amount of ozone produced by ultra-violet radiation and by auroral discharges.

The 678th meeting was held March 12, 1910, President Woodward presiding. Two papers were read:

Recent Work on Primary Triangulation in the Southwest: Mr. Wm. Bowie, of the Coast and Geodetic Survey.

After having completed the primary triangulation along the 98th meridian, in 1907, it was decided to extend the scheme from the 98th meridian, in central Texas, westward to the Pacific coast. This area in the southwest section of the country was badly in need of a control upon which to base surveys and engineering work.

It was originally intended that the portion of this scheme in the state of Texas should run along the Rio Grande River, from Brownsville to El Paso. This plan was abandoned owing to many difficulties which would have been encountered. The route used starts from the 98th meridian in the vicinity of Weatherford, and follows the Texas and Pacific Railroad across the state of Texas to El Paso, thence across the southern portion of New Mexico, Arizona and California to the Pacific coast triangulation in the vicinity of San Diego.

The reconnaissance for this scheme of triangulation, 1,224 miles in length, was done by a party under Mr. Bowie's direction, in four months and twenty-one days. The scheme consists of 92 primary and 38 secondary stations.

From Fort Worth to the Pecos River the land is rolling and very similar to that along the 98th meridian in Texas. From the Pecos River westward to the Pacific coast the country is mountainous with some peaks as high as 11,000 feet.

Upon the completion of the reconnaissance in February, 1908, the preparation of the stations for observations was begun at the eastern end of the line. Two seasons of observing have been completed, one of five months and three days, and one of four months and fifteen days; a total of nine months and eighteen days. The work done during those seasons was 72 primary stations occupied and completed, 12 primary azimuths observed and two base lines measured. The bases were about thirteen and fifteen kilometers in length. Six hundred and twenty-three miles of triangulation along the axis of the scheme were completed. The party doing this work was under the direction of Assistant J. S. Hill, except for two months of the first season.

The results show that the completed triangulation is of a grade equal to that of the best half of the primary triangulation previously done in this country.

Three fifty-meter nickel-steel (invar) tapes were used for measuring each base, and they gave very satisfactory results. Eight primary bases have been measured with invar tapes by the Coast and Geodetic Survey, during the past four years. These tapes hold their lengths well between standardizations. As a result of the use in the field,

no one tape has changed its length during any one season by as much as one tenth of a millimeter, or one part in 500,000.

A long step forward was made in geodesy when it was found that a primary base could be measured with steel tapes. But especial care had to be exercised with steel on account of the large coefficient of expansion, and all measurements were made at night. A second important advance was made in substituting nickel-steel tapes on account of their very small coefficient of expansion.

The observing on the Texas-California line of primary triangulation will be resumed in July of this year.

Field Observations in Iceland: Dr. F. E. WRIGHT, of the Carnegie Institution of Washington.

This paper dealt with the observations of a six-weeks' trip in Iceland by the speaker in the summer of 1909, especial attention being given to the physiographic and geologic features of the island and their influence upon the development of the country as a whole. The area of Iceland is about 40,000 square miles, and has about 6,000 miles of coast line. Its present population is about 90,000 people, largely an urban population. Iceland was settled by the Norseman in the ninth century, and for the first three hundred years thereafter it had a republican form of government. It is now a dependency of Denmark, but is largely self-governing.

Most of the houses in the country districts are built of peat, but in the towns corrugated iron is the chief material of house construction. The chief exports of Iceland are fish, eiderdown and ponies, many of the latter being used in the coal mines of England. 30,000,000 pounds of fish were exported from Iceland in 1901, most of which went to Spain.

Geologically, Iceland is a very young country, and for volcanic and glacial study it is the best region in the world, and affords the best idea of geologic forces. Iceland is a region of high seismicity and of much local magnetic disturbance.

R. L. FARIS,
Secretary

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

AT the 443d regular meeting of the society, held March 1, Mr. William H. Babcock presented a paper on "The Two pre-Columbian Norse Visits to America." So far no reliable evidences of Norse visits have been found on American soil, which is, however, not surprising in view of the great lapse of time, the small number of the visitors and the short duration of their sojourn. The records of the Norse visits are found in the saga of Thorfinn Karlsefin and the nearly identical saga of Eric the Red. The Flattoy book adds to the number of voyages, exaggerating many of the improbable features, and in other respects exhibiting signs of later development and corruption.

The lecturer gave an exhaustive survey and analysis of the sagas, subjecting their geographical, ethnological, historical and other data to a thorough and searching criticism. In conclusion, he said: "It seems clear that America was discovered. In addition we may be pretty safe in fixing on the neighborhood of the Bay of Fundy as the chief temporary home of Thorfinn's party in Wineland; and in following his route from Greenland thither, and later around Cape Breton into the Gulf of St. Lawrence and back to Straamfirth about as given. All else remains still open to discussion and more or less probable conjecture."

Mr. A. P. Bourland followed with an address on "The Study of Culture History in German and American Universities." The speaker gave a description of the "institutes" for the study of culture history established at some German universities. Such an institute is equipped with a series of libraries illustrating the development of the human race in all its aspects and directions, such as industry, arts and crafts, politics, jurisprudence, religion, etc. The creator of these institutes was Professor Carl Lamprecht, of Leipsic, whose conception of history is: The study of the development of human life on its economic and social sides.

AT the 444th meeting of the Anthropological Society, held March 15, Dr. Elnora C. Folkmar gave a lecture on "Education; Some Examples among Primitive Peoples." The field covered extended from Australia to Africa. The point brought out by the speaker was that among primitive peoples imitation and object lessons, as it were, take the place of methodical and theoretical teaching and training. The children unconsciously imitate the practises and doings of their elders and thus successively acquire what knowledge they have and need for life.

In the discussion Dr. J. R. Swanton called attention to the specialization in training among the Indians of the coast. Thus the Creek Indians have a kind of graded course of study, especially for the medicine men, with some sort of graduation marks by some insignia, such as a fox's skin,

the feather of a buzzard or owl. Mr. J. N. B. Hewitt pointed out that among the Iroquois education does not stop with childhood. The adults are trained in the knowledge of the tribal laws and customs and in what may be called intertribal law and diplomacy, such as the treaties and pacts entered by the tribe with other tribes, as also in the elaborate ritual connected with certain tribal events, such as the installation of new chiefs. Dr. J. W. Fewkes dwelt on education among the Hopi Indians.

I. M. Casanowicz,
Secretary

THE AMERICAN CHEMICAL SOCIETY
RHODE ISLAND SECTION

The regular meeting of the section was held February 24, 1910, at the University Club, preceded by the usual informal dinner.

Mr. C. E. Swett, of Providence, R. I., presented the paper for the evening on the subject "Field Notes from the Natural History of Silica." Mr. Swett first outlined the source and mode of formation of rocks in general and then took up the strictly silica rocks such as quartz, flint, etc. Finally he described the silica rocks containing metals, telling the chemical processes leading up to their formation, and showed a large number of specimens taken from various mines visited by him during the summer of 1909.

Albert W. Claflin,
Secretary

Providence, R. I.

THE AMERICAN CHEMICAL SOCIETY
NORTHEASTERN SECTION

The ninety-seventh regular meeting of the section was held at the Massachusetts Institute of Technology, Boston, on March 4.

Dr. Daniel F. Comstock, of the Massachusetts Institute of Technology, in an address on "The Present Conception as to the Constitution of Matter," briefly outlined the recent advances in the field of atomic and subatomic chemistry and physics, describing some of the brilliant experimental work that has marked these developments. There is a reasonable basis for believing that the atom has a real existence and is something more than a helpful fancy and also that the atom itself is a very complex structure.

Mr. M. C. Whitaker, of the Welsbach Co., Gloucester, N. J., described the monazite sand deposits of Carolina and Brazil, the methods of monazite mining and purification, and the preparation therefrom of the rare earths, with particular reference to the nitrates of thorium and cerium. The manufacture of gas mantles was described in some detail and there was indicated the probable lines along which improvements in mantles are likely to occur.

F. E. Gallagher

THE AMERICAN PHILOSOPHICAL SOCIETY

At the meeting of the society on March 18, the following paper was read by Dr. Jay F. Schamberg, of Philadelphia: "On Vaccination and on the Ravages of Smallpox among Royal Families." The speaker sketched the incidents of the discovery of vaccination by Jenner in 1798 and referred to the great importance of this discovery to the world. In the seventeenth and eighteenth centuries smallpox was an ever-present and death-dealing scourge, causing, it is estimated, 400,000 deaths a year in Europe. The visitations of this disease were severe in many royal families, particularly the Bourbons, the Hapsburgs, the Stuarts and the House of Orange. Since the discovery of vaccination, royalty appears to have been exempt from smallpox. Had vaccination been discovered a century earlier, the destinies of certain European countries would doubtless have been altered.

The address of April 1 before the society was delivered by David Fairchild, agricultural explorer in charge of foreign seed and plant introduction, U. S. Department of Agriculture, on "A New World for Exploration." With the origin of the term agricultural explorer it was recognized by the department that there is in the study of the botanical relatives of our cultivated plants a new world to explore. The botanical explorations of the past have been mainly for the purpose of classifying in a general way all the plant species of the world. Now that the possibilities of plant breeding are more fully recognized, the great importance of getting together the relatives of our cultivated crop plants has become very apparent. The importance was emphasized by the speaker of getting, before it is too late, the strains or races of well-recognized economic species which have been selected for centuries by cultivation in isolated mountain valleys, desert oases and oceanic islands. The rapid spread of railways and ocean travel and its accompanying seed exchange threaten to soon swamp these varieties, many of which may be of the greatest value to civilization.